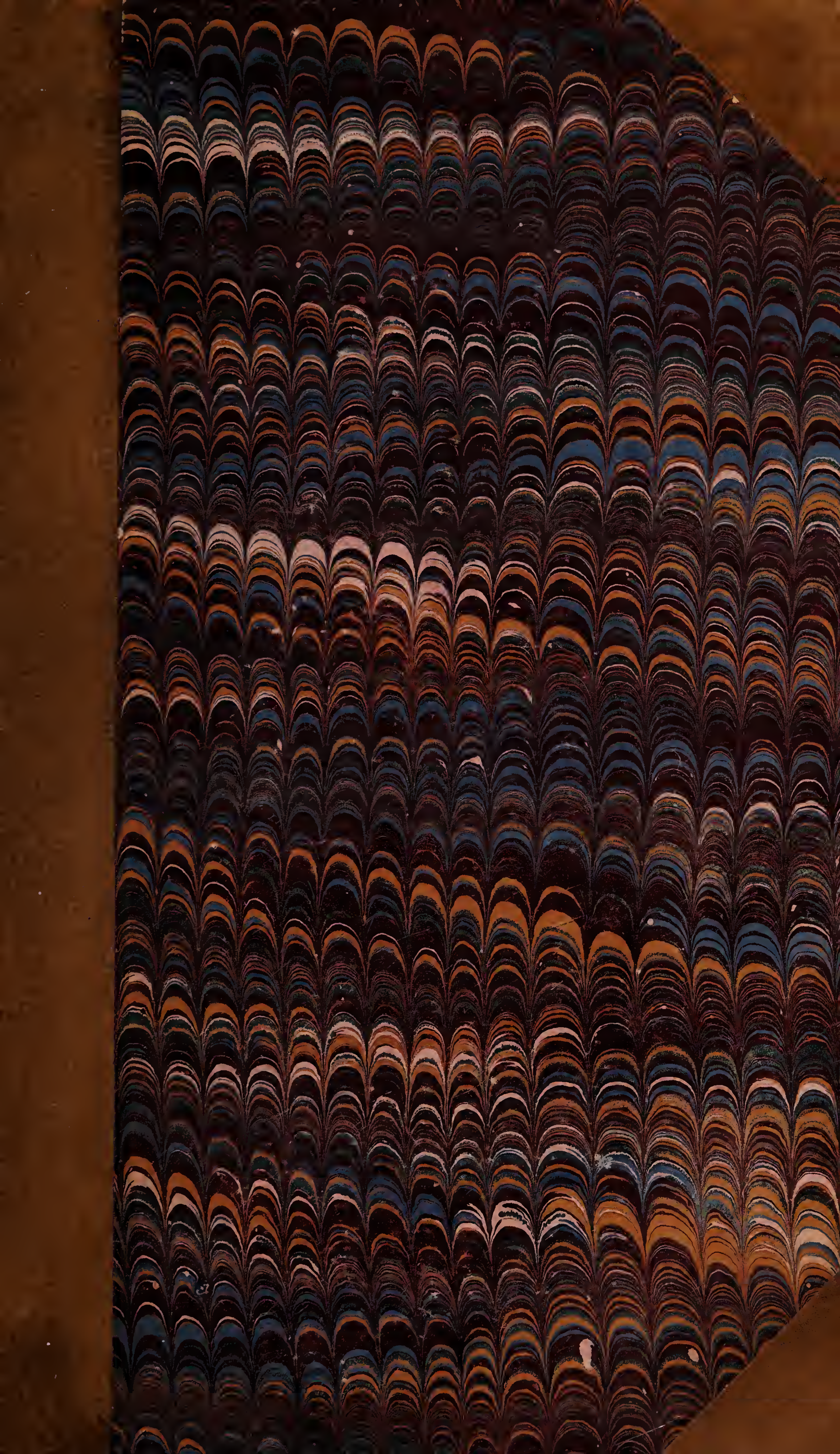

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EXPERIMENTAL
ESSAYS.

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EXPERIMENTAL
ESSAYS
ON THE FOLLOWING
SUBJECTS:

Contents

I. On the External Application of ANTISEPTICS in Putrid Diſeaſes.

II. On the DOSES and EFFECTS of MEDICINES.

III. On DIURETICS and SUDORIFICS.

By WILLIAM ALEXANDER,
SURGEON in EDINBURGH.

> Hæc autem ſola poteſt obtineri ratiocinatione exactâ, dum data experimenta, ſingulatim perfectè expenſa, in omnibus ſuis proprietatibus examinantur, dein inter ſe comparantur ſedulò, ut convenientia, vel diverſitas, pateſcat, tumque prudentiſſimâ fide notantur ea omnia, quæ in iis contineri perſpecta clarè inde deduci poſſunt.
>
> BOERHAAVE Inſtitut. Med. §. 24.

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THE

PREFACE.

THE experiments contained in the following Eſſays were begun ſeveral years ago for my own amuſement, and, I hoped, improvement. The greateſt part of them have been read before the Philoſophical Society of this city, and ſome of them before the Royal Society in London. All of them were made with as much accuracy as my time and judgment would allow of; and every phænomenon ariſing therefrom is related exactly as it appeared, as they were not deſigned to ſupport any hypotheſis already formed. Whether the concluſions

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drawn from them are juſt or not, I will not pretend to determine: this, however, I can ſay, that whereever they may be wrong, they are ſo from an error in judgment, and not from an intention to eſtabliſh a theory of my own, or miſlead the reaſon of others.

To the firſt eſſay on antiſeptics it may be objected, that it is incomplete, and wants ſeveral very eſſential experiments to confirm its utility. This is certainly true; but the reader will ſee I was at ſome pains to bring animals into a putrid ſtate, that I might thereby have an opportunity to make thoſe neceſſary experiments that are wanting. Could I have ſucceeded in this, or had I lived in a place where putrid diſeaſes are frequently to be met with, the eſſay had appeared in a different manner from what it here does. I now publiſh it as containing ſome hints to thoſe gentlemen under whoſe care putrid diſeaſes often fall, hoping they will make proper uſe of them, as every one muſt be convinced that the methods propoſed are at leaſt innocent.

The experiments on the doſes and effects of certain medicines, were made with a view of ſelecting the valuable from the uſeleſs,—determining the doſes and operation of the former,—and throwing the others into that contempt which they juſtly deſerve.

I had proceeded but a very ſhort way in this undertaking, when, as the reader will ſee, I had very ſufficient reaſons for deſiſting from proſecuting it any farther.

Out of four capital articles in the *Materia Medica* (which were all I made trials of) two were altogether uſeleſs, or very nearly ſo. And were the whole articles contained in it to undergo the ſame ſcrutiny, I am very much afraid, that more than a proportionable number of them would be found equally inſignificant.

The experiments on diuretics and ſudorifics were intended to clear up ſome difficulties which I had entertained concerning their action and effects on the human body. Thoſe on ſudorifics will furniſh ſome hints which I had not the moſt diſtant idea of; ſome of which will, I hope, if diligently attended to, prove very uſeful in practice. I am perſuaded they ought at leaſt to make us more attentive to the effects of ſweating than we have hitherto been, as they plainly prove that it is by no means an indifferent evacuation.

In the whole of what follows, I have carefully avoided giving my opinion dogmatically, by laying down invariable rules or precepts: ſuch having always appeared to me as ſo many chains to fetter the mind; as I have ever found, that he who paid the greateſt deference to them, has made the leaſt progreſs in ſcience in general. And I cannot help thinking, that they are as little applicable to that of medicine, as to any one whatever; for every one accuſtomed to attend the ſick, will often find caſes occur referable to no claſs of diſeaſes in any arrangement hitherto made, and conſequently not to be treated by any ſet of rules which are laid down. When this happens, the man who has ſtudied books and rules only muſt ſoon be at his *ne plus ultra*; whereas he who has carefully ſtudied Nature, formed a comprehenſive view of her general laws, and learned by an accurate reaſoning to trace effects up to their cauſes, will ſtill have it in his power to aſſiſt her, though directed to that aſſiſtance by no particular rule or precept.

To lay aſide a blind deference to rules, and encourage a free and liberal ſpirit of enquiry, are the only things that can give birth to improvement, and make truth emerge from that rubbiſh of error in which it is often buried. The preſent age has been remarkable for this freedom of enquiry; and if I among others have taken the liberty of uſing it, even ſometimes in oppoſition to great and celebrated names, it has always been for the ſake of what appeared to me truth. All the indulgence I can crave is, not to be condemned for ſo doing, till my reaſons have been heard and conſidered with the ſame impartiality as they are given.

Facts and experiments are the only true foundations of accurate knowledge, and the latter particularly are very much wanted in medicine. If thoſe which follow have any thing in them that merits the name of a diſcovery,—if they contain any thing worthy the name of an improvement,—or if they in any meaſure contribute towards the benefit of mankind, I ſhall think the labour they have coſt me very well recompenſed, though they were attended with no ſmall degree of danger to my own health; well knowing that it is the duty of every one, as far as is in his power, to promote the welfare of his fellow-citizens:

Non ſibi, ſed toti natum ſe credere mundo.

Edinburgh,
March, 1768.

ESSAY I.

On the EXTERNAL APPLICATION of ANTISEPTICS.

FROM the remoteſt ages of antiquity down to the preſent time, putrid malignant diſtempers have been the ſcourge of mankind. Fraught with contagion, they have often almoſt depopulated kingdoms, and always ſpread terror, death, and deſolation, around them, wherever they made their appearance. Various and ridiculous, according to the prevailing philoſophy or humour of the times, have been the antidotes contrived by the ancients to prevent them: and the remedies handed down to us, both for this purpoſe, and for that of curing them, are ſo unintelligibly compounded, or rather jumbled together, that they have long ſince fallen into that contempt which they juſtly deſerve.

Of late years, ſince we became better acquainted with the natural cauſes of things, ſeveral more natural methods and remedies have been contrived to prevent putrefaction. But theſe, however ſalutary, are ſhamefully neglected; and even when they are not, there are many circumſtances in life, which frequently counterbalance all their power, and bring on a putrid ſtate of the humours; which, to the great regret of every humane mind, proves too often fatal, in ſpite of all the efforts of the healing art.

Since the inveſtigation of antiſeptics, which have been found ſo numerous, and ſince the application of them to medicine, by the ingenious Sir John Pringle, it was natural to have expected, that a more ſpeedy and effectual method of curing putrid diſeaſes would ſoon have been diſcovered. But though this learned gentleman has furniſhed us with ſo large a ſtock of materials, we have hitherto made but very little progreſs in our methods of uſing them: and the reaſon appears to be, becauſe our whole attention has been employed about adminiſtering them internally, and we have wholly overlooked their external uſe; though it will appear plain, from ſome of the following experiments, that they may be conveyed much ſooner, and in much larger quantities, into the blood, when applied externally, than when taken into the ſtomach.

Sir John Pringle, as far as I know, was the firſt who attempted to ſweeten putrid fleſh by immerſing it in antiſeptics. Dr. Macbride has improved his hints, and not only ſweetened it by immerſion in the antiſeptics themſelves, but alſo by ſuſpending it in the ſteams which aroſe from them. It has been an eſtabliſhed fact theſe many years, that poultices of bark, or ſpirituous antiſeptic fomentations, applied to gangrened parts, have very much contributed to recover them. At preſent, almoſt every practitioner who attends the ſick in putrid diſeaſes, orders the room to be ventilated, waſhed with vinegar, or fumigated with aromatics: and what are all theſe, but ſo many methods of applying antiſeptics externally? I am perſuaded, that if we had reaſoned fairly upon them, they would have ſerved as ſo many leading hints to have diſcovered, that a human body may be ſweetened, and recovered from putrefaction, by being bathed in antiſeptics, as well as a part of any other animal.

Whether this will really be the caſe, is a queſtion that I am not at preſent furniſhed with a ſufficient number of experiments to determine: and as my practice affords but very little opportunity of ſeeing putrid diſeaſes, and conſequently of making the experiments neceſſary to elucidate this matter, I ſhall ſubmit to the judgment of the public thoſe I have already made. Several of them evidently prove that antiſeptics penetrate the ſkins of other animals as well as of men: that they enter immediately into, and circulate along with, the blood, and are by it diffuſed through the whole body. And ſince they evidently poſſeſs a power of recovering from a begun putrefaction, any body which they thoroughly penetrate; as they eaſily paſs through the human ſkin, enter the blood, and pervade the whole body; and as the application of them in this manner, in any ſtage of a putrid diſtemper, would, in my opinion, be very innocent, I think it highly worthy of the ſerious conſideration of thoſe who have it in their power to make the trial.

Experiments have fully demonſtrated to us, that we are poſſeſſed of the knowledge of many medicines which have a power of correcting putrefaction, when the particles of the corrector, and thoſe of the putrid body, can be brought into contact with each other*. The great deſideratum, therefore, in the cure of malignant diſtempers, ſeems plainly to be, the accompliſhment of this mutual contact of parts between the diſeaſed putrefying body and the corrector: which, in my opinion, will be much better done by applying the antiſeptic to the whole ſurface of the ſkin, by way of a bath, than by taking it internally; eſpecially when we conſider that the action of the ſtomach is ſo much debilitated in morbid caſes, and particularly in putrid ones, that but a very ſmall portion of the food, drink, or medicine, can be ſufficiently prepared by it to enter into the blood.

During a part of the laſt war I had ſeveral opportunities of ſeeing putrid malignant fevers among the ſoldiers and French priſoners; when I, almoſt conſtantly, obſerved that peculiar debility of ſtomach I have mentioned; inſomuch that there were but few patients who did not, ſoon after being attack'd, or in the more advanced ſtate of the diſeaſe, become almoſt incapable of retaining even the ſimpleſt food or medicine *: and, in theſe melancholy caſes, what could be done? The inefficacy of internal medicines appeared evident, as they were immediately thrown up: and yet no perſon, ſo far as I know, in ſuch circumſtances, ever thought of trying any other external ones than bliſters. A number of thoſe diſmal caſes made at that time ſo deep an impreſſion on my mind, that ſome time after I made it my particular buſineſs to conſider, whether there might not, in ſimilar circumſtances, be other methods tried to reſcue the miſerable victims from the jaws of death.

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* Vide Sir John Pringle's and Dr. Macbride's Experiments.

In proſecuting this inquiry, I took the firſt hint of uſing antiſeptics ex-

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* Dr. Auſtin, phyſician in Edinburgh, gave me an account of a putrid caſe he had lately under his care, where every thing was thrown up, almoſt the moment it entered the ſtomach.

ternally, from thoſe experiments which ſhewed that they were capable of ſweetening pieces of putrid fleſh when immerſed in them: and when I conſidered further, that ſpirituous aromatic fomentations, poultices, and cataplaſms of bark, and other antiſeptics, daily contributed to recover gangren'd parts, I was, from the whole of theſe reflections, led to think, that bathing the human body, when infected, in ſolutions or decoctions of them, might very poſſibly be of uſe, when internal remedies had either failed of ſucceſs, or could not be retained in the ſtomach. But as I was by this time confined to private practice, where few, or rather no really putrid diſtempers ever appeared, and had, conſequently, no opportunity of trying whether antiſeptics, when applied externally, would operate as I imagined; I determined to make ſome experiments with them, in order to throw all the light upon the ſubject that my ſituation would allow of.

EXPERIMENT I.

As Sir John Pringle and Dr. Macbride had both, by different antiſeptics, and in different manners, ſweetened parts of an animal which had become putrid; I reſolved to try if I could ſweeten a whole animal with the ſkin upon it; and, for this purpoſe, provided myſelf with a dead rat, which I kept till it was juſt beginning to putrefy, as I diſcovered by its ſmelling a little fœtid. I then boiled one ounce of bark in four pounds of water till one pound was conſumed, and in this decoction diſſolved three ounces of nitre. When the bath, thus prepared, came to the heat of 100 degrees of Farenheit's ſcale, I made a very tight ligature round the neck of the rat, to prevent any of the liquor from getting into its belly, put it into a glazed earthen veſſel, and poured the whole over it. At the end of ſix hours it was taken out, and was then perfectly ſweet.

EXPERIMENT II.

I kept another rat till it became conſiderably more putrid than the laſt, and then put it into a bath prepared exactly in the ſame manner. At the end of ſix hours it was taken out and waſhed, but ſtill retained its putrid ſmell. It was then returned into the ſame bath; and, at the end of ten hours more, was examined again: it ſeemed to ſmell rather leſs putrid. A freſh bath was then prepared, in which it was ſteeped for ten hours more. The freſh bath ſeemed to have operated very powerfully, for it now ſmelled much ſweeter. Into this it was put again for eighteen hours; and then, being examined, appeared intirely to have loſt its offenſive ſmell.

EXPERIMENT III.

A third rat was kept till it became ſtill more putrid than any of the former. It was then put into the bath, which was frequently changed during ſix days. At my firſt, ſecond, and even third examination, I doubted much whether I ſhould be able to recover it. At the fourth it did not ſmell quite ſo diſagreeable; and from that time the fœtorwent gradually off. At the end of the ſixth day, it was perfectly freſh.

EXPERIMENT IV.

A mouſe, which I kept till it became, as nearly as I could judge, as putrid as the rat, was ſweetened in the ſame manner, by repeated affuſions of a decoction of camomile flowers, in the ſpace of four days: and another by a pretty ſtrong ſolution of camphire in lime water, in about three days and a half. The ſolution of camphire was not ſo frequently changed as the decoction.

The laſt of the three rats which I recovered from putrefaction, was opened; and though the external parts of it were perfectly ſweet, yet, upon cutting it up, the inteſtines ſtill retained a ſmall degree of fœtor, and a very conſiderable degree of lividity, or rather blackneſs, appeared all over them. Upon ſteeping them about twelve hours in a bath of the ſame kind as that in which the rat had been, this remaining fœtor went entirely off, but the lividity remained ſtill the ſame. The two mice were likewiſe opened : their inteſtines were alſo livid, but perfectly ſweet. This does not ſeem to have been owing to any difference in the antiſeptics made uſe of, but to the mice being ſmaller, and more eaſily penetrated by the bath than the rat.

Theſe, and ſeveral other experiments of the ſame nature, gave me an opportunity of obſerving, that the antiſeptics, when applied to a dead animal, have a power to recover the whole, or any part of it, from a ſtate of putrefaction not too far advanced : yet they have no power of taking away that lividity or blackneſs brought on by the putrefaction. This conſtitutes a very material difference between what happens to a living and a dead animal recovered from putreſcency: for, when we recover any gangren'd part in a living animal, it is always in time reſtored to its natural colour; whereas theſe experiments ſhew, that the putreſcency in a dead animal may be intirely remov'd, and yet the diſcoloration remain when it is perfectly ſweet.

Lividity on a living animal ſeems, as far as I can obſerve, to ariſe either from an extravaſation of blood happening in conſequence of ſome violence done to the ſolids by external force, whereby they are ruptured, ſo as to allow their contents to paſs into the interſtices of the muſcular fibres; or in conſequence of an inflammation, when the red globules of blood are violently puſhed into the lymphatics. In both theſe caſes the ſtagnating blood ſoon loſes its natural colour, becomes firſt livid, and afterward black. But in a dead animal, ſo far as I have been able to diſcover by diſſections, the firmneſs of the ſolids was always very much deſtroyed, and the lividity ſeemed to have ariſen from the fluids and ſolids having joined together to conſtitute an indiſtinct and grumous maſs: and this I imagine will lead us into the reaſon, why the natural colour is reſtored to a livid part of a living animal when recovered from putrefaction, and not to that of a dead one. For, in a living animal, the ſolids being generally unhurt, the extravaſated matter is taken up by the abſorbents, and enters again into the blood: but when it happens that the ſolids come to be affected alſo, the whole morbid part is then ſeparated from the ſound body by means of ſuppuration; whereas, in a dead body, the ſolids and fluids being both equally affected, and no circulation going on, nor any active power exiſting to throw off the diſeaſed part, the colour once loſt can never be regained, as we can never unmix, and reſtore to their proper places, the ſolids and fluids, upon which this natural colour ſeems very much to depend. All that we can therefore

fore do in this caſe is, by the application of antiſeptics, to put a ſtop to that fermentative putrefaction, whereby the ſolids and fluids are blended together into a maſs.

Here it will be natural to inquire, why that particular ſpecies of fermentation which brings on putrefaction, though it be the ſame both in living and dead animals, ſhould in the latter almoſt always affect the fluids and ſolids, at the ſame time; and in the former, often leave the ſolids in their natural ſtate for a long while after the fluids have been affected. This appears to be moſt naturally accounted for, by conſidering that putrefaction never happens in a living animal but by extravaſation *; and in a dead one

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* I do not mean by this, that no part of a living animal ever putrefies; for where there is an extravaſation, it happens often otherwiſe: but then that part of the animal is dead before the putrefaction takes place, being without ſenſation and circulation; the only ſure marks to diſtinguiſh a living part from a dead one.

always without it: for, unleſs ſome violence is done to the creature before it expire, ſo as to induce the extravaſation, moſt of its humours ſoon after coagulate, and then it cannot poſſibly happen. It would be foreign to my preſent purpoſe to attempt to explain the cauſe why ſtagnating humours putrefy. It is ſufficient for me here to know that it is a certain fact, and that it generally takes place in the extravaſated humours of a living animal, while the ſolids immerſed in this extravaſation are, perhaps, preſerved, by having ſtill their own fluids circulating through them: nor is this to be wondered at, when we conſider that fluids conſtitute a very large ſhare of even the denſeſt parts of our bodies. If this be allowed as the reaſon why the ſolids of a living animal remain often a long time intire amidſt ſtagnant putrid fluids, the want of it will eaſily explain why the ſolids and fluids of a dead animal, being one ſtagnated maſs, ſhould be equally liable to be acted upon by a putrefying cauſe.

EXPERIMENT V.

If putrefaction be too far advanced before any attempt is made to ſtop it, in that caſe, no whole animal, nor any part of it, can ever be recovered. I allowed a rat to grow conſiderably more putrid than any of the former; but all the methods I could uſe did not ſeem in the leaſt to have ſweetened it; though, indeed, they retarded the progreſs of the putrefaction, and kept the animal pretty nearly in the ſame ſtate in which it was at the beginning of the experiments. But there is a ſtate of putrefaction, a few degrees beyond this, which it is impoſſible even to retard, and where no methods can ſave the texture of the parts from running into, almoſt, immediate diſſolution. This ſhould teach every one always to call in proper aſſiſtance, as ſoon as poſſible, in putrid diſtempers; for, in their firſt ſtage, they will, perhaps, eaſily yield to judicious remedies; in their ſecond, the caſe is at beſt but doubtful; and in the laſt, the patient is always irrecoverably loſt.

EXPERIMENT VI.

I took a ſmall rabbet, and having killed it, put it up to the middle into a very ſtrong ſolution of nitre, and kept the upper part of it carefully above the ſurface of the liquor. In this manner it remained for twelve hours, during which time the ſolution was kept in a heat of about 96 degrees. I then took it out of the bath, ſkinned it, and cut off two drachms of its fleſh from that part which had been immerſed in the ſolution, and the ſame quantity from that part which had been kept above the ſurface of it. Theſe pieces were each put into a ſeparate gallypot, with two ounces of pure water, and ſet in a heat of 96 degrees. After they had ſtood 24 hours (which is much longer than the time uſually required to produce putrefaction in that degree of heat), the piece cut from that part of the rabbet which had been above the ſurface of the bath, began to putrefy, but the other piece was not changed till ſix hours after; and even then the putrefaction advanced much ſlower in this, than it did in the other.

EXPERIMENT VII.

I took two living rabbets, nearly of an equal ſize, and having diſſolved ſix ounces of nitre in twelve pounds of water, and heated the ſolution to 110 degrees, put one of the rabbets into it, and confined it there for the ſpace of fifteen minutes; taking care always to keep its head above the ſurface of the liquor, that none of it might enter by its mouth. The creature did not ſhew any ſigns of uneaſineſs in the bath, and, as ſoon as it was taken out of it, ran about the room in its uſual manner. Eighteen hours after I heated the ſame ſolution to the degree of 105, and put the ſame rabbet into it for the ſpace of half an hour; toward the end of which time it ſeemed very uneaſy, and I imagined it was ſick; but as ſoon as it was taken out it appeared perfectly well, and immediately eat ſome of its uſual food. Two hours after this, it was killed; a piece of paper was ſteeped in the ſerum of its blood, then dried by a ſlow fire, and expoſed to the flame of a candle, when it immediately caught fire, ſparkled, and emitted a bright flame like nitre; a ſure ſign that the blood was impregnated with that ſalt. The other rabbet was killed at the ſame time, and they were both ſkinned and hung in a cool cloſet, a yard diſtant from each other. After they had hung four days, they began both to ſmell a little fœtid. On the ſixth day, lividity, and other ſymptoms of putrefaction, were very evident on the neck, and even appeared faintly on ſeveral other parts of the rabbet that had not been bathed. Some ſmall degree of lividity was alſo viſible on the neck of that one which had been bathed; but none could be diſcovered on any other part of it, nor did it ſmell half ſo diſagreeably as the other*. Both theſe rabbets were kept for about three weeks longer; when, inſtead of running into a total diſſolution as I expected, they grew ſo extremely dry that the putrefaction advanced but very ſlowly; however, at the end of that time, the one which had not been bathed, was evidently much more fœtid than the other.

The ſkins of rats, of mice, and of rabbets, are all very cloſely covered with hair; and, as appears by a microſcope, not nearly ſo porous as thoſe of men. If, therefore, under theſe diſadvantages, the two former abſorb a ſufficient quantity of an antiſeptic to recover the animals from a begun putrefaction; and the latter, enough to keep it much longer than uſual, from becoming in any degree putrid; it is certain, that ſtill a larger quantity will go through the human ſkin; ſo that, if the effect of an antiſeptic be any way proportioned to its quantity, we have much more to hope for from its operation on the human ſubject, than on any of theſe animals.

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* Their inteſtines were taken out as ſoon as they were killed.

EXPERIMENT VIII.

I took two living rabbets, made a ſmall inciſion in the thigh of each of them, and filled the inciſions with putrid matter from a piece of mutton, which had been long kept in a phial for that purpoſe. On the third day after the operation, both their wounds appeared ſomething livid; on the fourth, the lividity was leſs perceptible, and they were covered with an eſchar; and on the ſeventh,

venth, they were perfectly healed. I then made a new inciſion on the thigh of each of them, ſomewhat larger than the former, and filled each with a piece of the ſame mutton when it was become extremely fœtid, applying over them ſtrong pieces of ſticking-plaſter. After thirty-ſix hours the dreſſings were removed, when the wound upon one of the rabbets was full of a ſtinking, ſanious, matter, with a dark livid ring around it. The next day this lividity was ſtill more dark, and the diſcharge from the wound appeared exactly like that from a gangrened part. On handling it, the rabbet ſeemed, by its cries, to feel very great pain; but, notwithſtanding all this, to my great ſurprize, it began the following day to ſuppurate, and, in about five days more, was perfectly healed. The plaſter had ſlipt off from the wound on the other rabbet; it was therefore inoculated again. The wound ſhewed much the ſame ſymptoms as that on the other, and healed nearly in the ſame time.

The intention of this experiment was to have thrown both the rabbets into a putrid fever, and then to have attempted the cure of one of them by the external, and of the other by the internal, application of the ſame antiſeptic; and to have obſerved carefully, in which of theſe ways it ſucceeded beſt. In order to bring on the putrefaction, and make my experiment ſucceed, I had the rabbets fed on bread and milk, leſt the antiſeptic power of green vegetables, their natural food, ſhould overcome the ſeptic power of the putrid matter.

Nothing can demonſtrate more clearly than this experiment, what nature is able to perform, in an animal that lives agreeably to her dictates, and whoſe blood is not vitiated by irregularity or debauchery; for though the matter with which the rabbets were laſt inoculated, was ſo extremely fœtid, that when I took the cork out of the phial in which it was contained, I was obliged to hold it below the chimney, otherwiſe the room was in an inſtant filled with ſuch an intolerably offenſive vapour that it was impoſſible to remain in it; and though it evidently affected the parts to which it was applied in both the inoculations, yet nature was ſtrong enough either to keep it from entering into and contaminating the blood, or to throw it off again by ſuppuration.

EXPERIMENT IX.

Having, by the foregoing experiments, fully ſatisfied myſelf that diſſolved antiſeptics penetrated the ſkins of dead and of living animals; I next reſolved to try if I could determine nearly the quantity that would be abſorbed by the whole ſurface of a human body, when the ſolution was of a given ſtrength, and applied for a given time. For this purpoſe I diſſolved four ounces of nitre in four pounds of water, and heated the ſolution to 100 degrees of Farenheit's ſcale. I then rubbed one of my hands with a hard cloth, put it into it as far as the junction of the carpal bones with the radius and ulna, and kept it there for fifteen minutes. When this time was elapſed, I took out my hand, weighed the bath, and found that it had loſt an ounce and a half *. I next evaporated the water over a ſlow fire, and ſet the nitre to chryſtallize. When the chryſtals were properly ſeparated from the remaining water, they weighed only two ounces. The ſurface of my hand had imbibed no more than an ounce and a half of the ſolution, and yet two ounces of the nitre, which conſtituted a part of it, were loſt; which exceeded by half an ounce the whole quantity abſorbed, and made me ſuſpect, what I found by a ſubſequent experiment, that the nitre, as well as the water, had evaporated in the

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* Including what was loſt by evaporation.

boiling: I therefore concluded, that only a quantity of it proportionate to the quantity of water in which it was diſſolved, could be abſorbed. Allowing this to be the caſe, which I think cannot eaſily be denied, it will appear from a fair calculation, that a much larger quantity of this or any other diſſoluble antiſeptic ſalt can be thrown into the blood in this manner, than can be taken with impunity into the ſtomach. Beſides, this method of application has this peculiar advantage, that all which is abſorbed goes immediately into the blood; whereas we cannot reaſonably ſuppoſe, that all of it that is taken into the ſtomach can poſſibly do ſo.

The calculation of what the whole body would abſorb, from what was abſorbed by one hand, is as follows: When one ounce of nitre is diſſolved in one pound of water, the proportion of nitre to that of the water, is nearly as one to ſixteen; and therefore, every ounce of water contains nearly half a drachm of nitre. One ounce and a half of the fluid was abſorbed by my hand, which ounce and a half contained forty-five grains of nitre. Now, allowing that the ſurface of my hand is to the ſurface of my whole body as one is to ſixty (which is a very moderate computation); and taking it for granted alſo, that all the ſurface of my body will abſorb equally with that of my hand (which it certainly will do at the leaſt, as it is conſtantly covered, and on that account more porous than my hand, which is almoſt always expoſed to the air); it follows, that if my whole body had been immerged for the ſame ſpace of time, in a ſolution of the ſame ſtrength, it would have abſorbed ten pounds five ounces of it; and this ten pounds five ounces would have contained 2700 grains, that is, five ounces five drachms of nitre, which is indeed a very large quantity. But if the ſolution was made ſtronger, a quantity ſtill much larger might be imbibed in the ſame manner. It may, indeed, be objected, that even this quantity received immediately into the blood would, perhaps, prove fatal; or, if not ſo, that it would, at beſt, be a dangerous experiment to attempt it. But, in my opinion, there is very little harm to be dreaded from it; and if there is, a ſolution of whatever ſtrength we pleaſe, can eaſily be at any time prepared, the uſe of which can be productive of no miſchief. Or a decoction of bark, or ſome other antiſeptic vegetable ſubſtance, may be uſed inſtead of the nitre; and then it is impoſſible that we can have any thing to fear. Tho', even ſuppoſing the experiment to be dangerous, I think the known fatality of putrid diſeaſes would fully authoriſe a perſon to make it; for it is certainly much better to try every thing which has the ſmalleſt chance in deſperate caſes, than to abandon a patient to certain death.

EXPERIMENT X.

In order to diſcover how I had loſt a greater quantity of nitre in the laſt experiment,

periment, than the quantity of the bath that was abſorbed, I again diſſolved four ounces of it in four pounds of water; and, without bathing any thing in it, ſet it immediately to evaporate over a ſlow fire. In the vapour ariſing from it I ſuſpended ſeveral pieces of paper, at different diſtances from its ſurface; and, when they were thoroughly wet, I dried them, and having expoſed them to the flame of a candle, found them all equally impregnated with nitre; a demonſtrative proof that it evaporated along with the water. When the evaporation was finiſhed, the nitre was ſet to chryſtallize: on weighing it, I found one drachm more than in the laſt experiment; but no inference can be drawn from this difference, as a quicker or ſlower fire might eaſily produce it.

EXPERIMENT XI.

In the ninth experiment I had loſt a certain quantity of the nitre made uſe of, but had no demonſtrative proof that any part of what was ſo loſt had gone into my blood. I therefore prepared a ſolution of it, of the ſame ſtrength as in that experiment; and having heated it to 100 degrees, put both my feet into it, and kept them there exactly fifteen minutes. In about ten minutes after they were taken out, I had a very plentiful diſcharge of urine, in which I wetted ſome pieces of paper; and having dried, and expoſed them to a flame, found them all very highly impregnated with nitre. Some experiments, to be mentioned afterwards, will ſhew this ſalt to be a very powerful diuretic; but I do not recollect that ever the internal uſe of it made me evacuate ſuch large quantities of urine as I did by this bath, though I had then drunk no remarkable quantity of any liquid. It would therefore ſeem, that, when uſed in this manner, it has a greater tendency towards the kidnies than when taken internally: but this a ſingle obſervation does not authoriſe me to aſſert. I have likewiſe found, that the urine may be impregnated with nitre taken into the ſtomach; but, in this way, the quantity it contains is much leſs; nor does the impregnation take place, ſo far as I have obſerved, till at leaſt two hours after the ſalt is taken; whereas, in this experiment, it took place in twenty-five minutes.

EXPERIMENT XII.

The laſt experiment afforded a demonſtrable proof, that diſſolved nitre, and, conſequently, every other ſoluble ſalt, might be taken-up by the abſorbent veſſels, and introduced into the blood; but I had hitherto attained no evidence ſufficient to ſatisfy me, that the particles of any antiſeptic vegetable, in a decoction, or any other form, could enter in the ſame manner: I therefore poured three ounces of freſh urine into a phial, and put into it two drachms of mutton. Three ounces of urine, evacuated at the ſame time, were put into another phial;

and they were both ſet in a heat of about eighty-four degrees, at four o'clock in the afternoon. I then prepared a very ſtrong decoction of Peruvian bark, heated it to 100 degrees, and that ſame evening, at eight o'clock, put both my feet into it, and kept them there for an hour and a half: at half an hour after ten o'clock having made urine, I put three ounces of it into a phial, with one drachm of the ſame mutton as was in the other, and three ounces into a phial by itſelf. Theſe two laſt glaſſes I ſet in the ſame place with the former, having previouſly marked them, to diſtinguiſh them from each other. At the end of 28 hours, the urine that had been made before I bathed my feet in the decoction, began to have the diſagreeable ſmell peculiar to urine turning ſtale; this ſmell augmented the ſecond and third day after; when, being fully convinced that it was putrid, I threw it out. The other glaſs, which contained the urine that had been made before the bathing, and the drachm of mutton, continued perfectly ſweet till about the end of the third day, and then began to emit a ſmell reſembling that of ſtale urine and putrid meat, which continued encreaſing for ſeveral days: the mutton was then taken out: it was ſoft, ſpongy, and would hardly bear to be handled without falling to pieces. From the firſt day the contents of both theſe glaſſes always appeared turbid from top to bottom, and of a whitiſh colour.

After eight days, the urine which had been made after I bathed, and which had the mutton in it, began to ſmell a little: but it was ſeveral days more before I could be ſenſible that this ſmell was encreaſed. I kept it by me fourteen days, and ſtill the putrefaction was very trifling: on examining the mutton, its texture, colour, and firmneſs, were found very little injured.

The urine which had been made after bathing in the bark, remained perfectly ſweet, without any ſediment, and free from the leaſt urinous or putrefactive ſmell for the ſpace of three weeks: the glaſs was then overturned by an accident, which had happened for ſome days, before I knew any thing of it. But ſome urine which I paſſed the next morning after the bathing, remained in the houſe perfectly ſweet for upwards of five weeks; during all which time it never depoſited any ſediment, but contracted a white cruſtaceous ſurface, and left ſomething of a gummy adheſive nature round the brim of the bowl in which it had been kept.

In a ſmall quantity of this urine, after it had been kept about a month, I diſſolved a few grains of ſalt of ſteel. The ſolution was of a turbid greyiſh colour, and words written with it on a piece of white paper, were very legible, being of a dun black colour. I at firſt took this as a proof that the urine was ſaturated with bark; but to my great ſurpriſe, on trying the ſame experiment with freſh urine, the effect was exactly the ſame.

Though this experiment does not afford a plain demonſtration of the bark having entered into my blood, yet it approaches as near to it as poſſible. For the urine which I made before the bathing, began to putrefy nearly in the ordinary time that urine takes to run into putrefaction: whereas that which was made immediately after the bathing, and that which was made the next morning after it, did not turn putrid while I kept them. To what could this be owing but the bark? Is it reaſonable to ſuppoſe, that any cauſe could exiſt in my body, which could make a quantity of urine, made at four o'clock in the afternoon, putrefy in about 24 hours; and another quantity, made about ten o'clock that ſame night, reſiſt putrefaction during the ſpace of three weeks; and another, made next morning, reſiſt it five weeks? Surely this is not to be explained upon any other hypotheſis, than that of the bark having entered into my blood, and been ſeparated along with my urine.

One phenomenon that happened in this experiment, ſurpriſed me not a little: It was, to find one part of the urine made before the bathing, putrify much ſooner alone, than another equal part of the ſame urine, with a piece of mutton in it. This is quite contrary (as far as I know) to what uſually happens to every other fluid liable to putrefaction: for all the obſervations hitherto made, agree, that theſe run much ſooner into that ſtate, when any animal ſubſtance is added to them, than when alone; and even many fluids, of themſelves not putreſcible, may be rendered ſo by the addition of any animal ſubſtance. But here it would ſeem, that the putrefaction of the urine was very conſiderably retarded by the mutton that was put into it.

It has long been an eſtabliſhed opinion, that neutral ſalts are the only medicines that can enter into the blood, paſs through the body, and ſtill retain their priſtine nature, and be reduced to their original form. But from what happened here, it may be concluded that bark is capable of mixing with the blood, and ſtill retaining its antiſeptic power: and if it can paſs through the blood with this power, it may perhaps alſo be reducible to its original form. On a ſtricter inquiry it will probably be found, that there are other things of the ſame nature which have hitherto eſcaped our notice.

EXPERIMENT XIII.

As the laſt experiment had not fully ſatisfied me, whether the bark had entered through the cuticular pores into my blood, I imagined if it would cure an ague by bathing in a decoction of it, there would remain no more doubt concerning the matter. I had no ſmall difficulty to get a proper patient for an experiment of this kind, as agues are very rarely met with in this metropolis; and as it was neceſſary to find one who, ſince the appearance of the fit, had taken no medicine. However, at laſt, having met with a labouring man in the ſuburbs, who had ſuffered four regular fits of a tertian, I with much difficulty obtained his conſent, after I had previouſly explained to him my reaſons for the experiment, ſhown him that no danger could poſſibly ariſe from it, and given him money to buy a pound of bark out of a laboratory, that he might be certain that I had mixed nothing with it.

Matters being thus far ſettled, I left nim directions to boil it in a large kettle of water for four or five hours, and then to ſend for me, which was done accordingly. When I came, I ordered a deep narrow tub to be got, reduced the heat of the decoction to 100 degrees of Farenheit's ſcale, poured it into the tub, and made the patient rub his legs ſtrongly with a hard cloth, and put them into it. A cloth was laid over the mouth of the tub to detain the vapour, and the liquor kept, as near as poſſible, to this original heat for two hours; then the patient was taken out and put to bed. This firſt bathing was in the evening, after the fit for that day was over. He was ordered to repeat it again the next day, and to begin about three hours before the time that he expected the paroxyſm to return. He did ſo; and ſoon after he came out of the bath, grew ſick, and went to bed, but had very little either of the cold or hot fit. He repeated the bath again that evening, the next morning, and the next night. I then ordered him to deſiſt from uſing it, but to keep the liquor by him, which he did, and paſſed four days at his ordinary labour in perfect health; but the fifth day, having got wet to the ſkin, his ague returned in the evening. As ſoon as the fit was over, he heated his liquor, and bathed in it as formerly; and afterwards, by my direction, took two vomits, and continued to bathe twice every day for the ſpace of four days. He has had no return of the fit after uſing the bath this time, though about two months have elapſed ſince he left it off.

It is impoſſible for any thing to have afforded a clearer proof of the bark having entered through the ſkin into the blood, than this; for we know it has a ſpecific power of curing an ague, and is the only thing that is poſſeſſed of this power. It was here applied to the ſkin, the ague was removed, and conſequently it muſt have penetrated the ſkin and entered into the circulation. If the ague, ſo removed, had not returned again, it might have been objected that its diſappearing at the time it did, was only fortuitous; but its returning again, and being removed a ſecond time, in the ſame manner, leaves no room to doubt, that both theſe removals were owing to the action of the bark. It is no uncommon thing for agues which have for ſome time diſappeared by the internal uſe of the bark, to return, in conſequence of its not being long enough continued, or taken in too ſmall doſes. The very ſame thing happened here; and as a longer continuance of the bathing had the ſame effect as a longer continuance of the internal uſe of the bark would have had in the ſame circumſtances, we have here an evident demonſtration, that the particles of an antiſeptic vegetable, properly prepared, can gain admittance into a living animal, through the pores of the ſkin.

This is a fact which has long been known; though it appears that very little uſe has hitherto been made of it. In the countries where agues are endemic, and where even children are ſubject to them, who are too young to be prevailed upon by argument, or urged by force, to take ſo unpalatable a drug as the bark; ſome practitioners have applied it to the ſurface of the ſkin in various forms, ſuch as plaſters, poultices, and even the dry powder quilted between the folds of a waiſtcoat made for that purpoſe. All theſe, and ſeveral other ways, I am informed, have been attended with ſucceſs: and they all afford corroborating proofs, that the virtues of very fine vegetable powder may be received through the ſkin; but when this vegetable powder is ſtill further broken down, by being prepared into a decoction, it is certainly preferable to any of the above preparations, both in agues and in all putrid diſtempers.

Not to inſiſt fully, at preſent, on all the inferences which may be drawn from a certainty of the bark penetrating the ſkin, and curing an ague when externally applied, I ſhall only mention, that I have ſeveral times met with patients who, from a repeated uſe of it, had contracted ſuch an unconquerable averſion to it, that rather than ſwallow an ounce of it, they would have ſubmitted themſelves to any trouble or expence whatever. When theſe caſes occur, this experiment opens to us a method of relieving the patients by the ſame medicine, without ſubjecting them to the diſagreeable taſk of ſwallowing it.

The ingenious Dr. Francis Home, in his *Principia Medicinæ*, is of opinion, that relaxation of the animal fibres is the cauſe of an intermittent fever; and his reaſons are, *Quia*, 1mo, *veniunt temporibus annis humidis.* 2do, *Aufugiunt temporibus ſiccis.* 3tio, *Quo magis humidum tempus, eo magis ſæviunt.* 4to, *In locis aquoſis, plaudoſis, ſemper graſſantur.* Theſe appearances, he thinks, are to be accounted for from moiſture rendering the fibres longer and leſs elaſtic: and from the whole very juſtly infers, that as agues are cured by warm aſtringent medicines, theſe medicines operate only by removing the relaxing cauſe.

On mentioning my intention of making this experiment to the Doctor, he was of opinion, that if it ſucceeded, it would overturn his theory of relaxation, as the warm bath is known to relax more powerfully than any other thing we know. It has ſucceeded; but, notwithſtanding that, I am inclined to think, has not invalidated any thing which the Doctor has advanced. For when a warm bath, prepared with ſo ſtrong an aſtringent as the bark, is applied, the relaxing quality reſiding in the heat and moiſture may be, and certainly is, counterbalanced by the aſtringency of the medicine: and this ſeems to be confirmed by ſteeping a piece of leather in a decoction of oak, or Peruvian bark, heated to 100 degrees; as the leather here does not come out with its fibres relaxed and elongated, but has them contracted and ſhrivelled up in a very evident manner: which plainly points out to us, that the power of aſtringency is not deſtroyed by the moderate heat of the vehicle in which the aſtringent is conveyed. After bathing my own legs and feet in a decoction of the bark, I felt a contraction of the ſkin, ſomething ſimilar to what happens in leather; from whence we may infer, that the bark in a warm vehicle will operate in the ſame manner on a living as on a dead animal.

My intention in making this experiment, was not with a view to introduce a cuſtom of curing agues by any external application; I am conſcious that it would be attended generally with too much expence, and always with a trouble which few people would ſubmit to. Beſides, it has not, perhaps, advantages enough over the internal method, to deſerve to be preferred to it. What I had chiefly in view, was to diſcover a method of introducing a large quantity of any antiſeptic more immediately into the blood, in putrid diſeaſes, than when taken by the ſtomach; which I looked upon as a conſiderable improvement in medicine: and I hope I have, in ſome meaſure, obtained my wiſhes.

The only objection I have ever heard made againſt the external application of antiſeptics in this manner, is the heat of the bath; for any heat near 100 degrees, has been found, by a variety of experiments, to conduce very much to putrefaction in dead animals, or mixtures of animal and vegetable matter ſet to putrefy. But this objection will loſe much of its weight when we conſider, that though the natural heat of the blood in the human ſubject in perfect health, is about 98 or 99 degrees of Farenheit's thermometer, and in ſome animals much higher; and in a human ſubject in a fever, though it often ariſes to 112, or upwards; yet no putrefaction enſues: whereas the ſame degree of heat will greatly accelerate it in any dead animal.

I have, indeed, always looked upon much heat as very deſtructive in all fevers,

vers, eſpecially in putrid ones; therefore, where this kind of bathing becomes neceſſary in them, could it be ventured on with ſafety below the heat of 100 degrees, and could it at any inferior degree have the ſame chance of penetrating the ſkin, I ſhould think it much more adviſable to order it ſo. Not becauſe I am afraid, that putting a perſon into a bath heated to 100 degrees, can very much augment his heat; for experience teaches us, that the ſame perſon put into a warm and a cold bath, feels much leſs heat after he comes out of the warm, than out of the cold one: and that this degree of heat is actually leſſened, appears evidently by the application of a thermometer to any part of his body.

Experience alſo teaches us, that the ſame degree of heat applied with and without moiſture to any body, will not equally augment the heat of that body. The dry heat bracing up the fibres will, of conſequence, augment the velocity and momentum of the blood and heat depending thereupon much more than the moiſt one, which, by relaxing them, will diminiſh this velocity and momentum. For all which reaſons, when a bath of this kind becomes neceſſary, I think we have very little to fear from heating it to 100 degrees; becauſe, if the heat of the perſon to whom we apply it is then above that degree, the bath will in that caſe act as a cooler, and contribute to reduce a heat which is too great; and if it is below that degree, the patient cannot ſuffer much by having it raiſed to it.

Many and various have been the experiments made to determine the degree of heat that ſooneſt induces putrefaction in dead animals and other putreſcible ſubſtances; and by analogical reaſoning, the ſame degree of it which has been found to have this effect ſooneſt on a dead animal, has been ſuppoſed to have the ſame alſo on a living one. It has therefore been dreaded as highly deleterious, and carefully avoided in all caſes where a putrid diatheſis of the blood was ſuſpected.

Analogical reaſoning will often miſlead the attentive, and almoſt always the inattentive, inquirer. In the caſe before us, one very material circumſtance ſeems to have been intirely overlooked, which is, that a degree of heat abſolutely neceſſary to life, in many living animals, is ſtrong enough to make almoſt every dead one ſoon run into a ſtate of putrefaction: the human ſubject, for inſtance, in perfect health, is of a degree of heat which will make the ſame ſubject after death putrefy in a few hours; and domeſtic fowls are of a degree which will ſtill more quickly deſtroy and ſpoil one of them, or any other animal after death; from the heat, therefore, that will ſooneſt of any other bring putrefaction on a dead animal, hardly any tolerable gueſs can be given at that which will have the ſame effect on a living one. For this reaſon we ought not to make our obſervations on what paſſes in a body after death, and transfer them to what paſſes in the ſame, or any other, when in life; but in order to come at the truth of this matter, our obſervations ought to be taken not only from the heat that brings a living animal ſooneſt into a putrid ſtate, but from that which brings the human ſubject ſooneſt into it.

Little or nothing ſeems hitherto to have been attempted to make this diſcovery; and moſt of the authors whom I have had an opportunity of looking into, have either paſſed it over in ſilence, or have ſaid nothing ſatisfactory concerning it. Dr. Shebbeare, who, on account of the ſatyrical manner in which he has treated almoſt every author he has mentioned, has not attained that credit he would perhaps otherwiſe have had, is the only one I have met with who affirms, that the degree of heat in putrid diſeaſes, eſpecially towards their laſt ſtages, is always leſs than what is natural to the conſtitution in perfect health *; on which

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* Since the writing of this, I have been informed, that there is lately publiſhed, ſomewhere in Germany, a diſſertation intitled *De Calore*, which

account he exclaims bitterly againſt Boerhaave, for inferring from an experiment he made, that a very great degree of heat is the cauſe of animal putrefaction.

It has long been an obſervation, that heat does a great deal of miſchief in putrid diſeaſes, and that cold contributes to their recovery; but, as I juſt now hinted, no attempt has hitherto been made to aſcertain the exact degree of heat that either brings putrefaction on a living animal, or that proves moſt favourable to the increaſe of it after it is begun. I have known patients, at different times and in different diſeaſes, at all the different degrees of heat between 84 and 112, in whom no viſible ſymptoms of putrefaction enſued; and I have known patients in highly putrid diſtempers at ſeveral of theſe intermediate degrees: certainly this is a proof that putrefaction is at leaſt confined to no degree of heat, and that it can alſo exiſt in a leſs

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aſſerts the ſame thing, and intirely overturns the Boerhaavian doctrine, of heat being the cauſe of putrefaction.

degree of it than many other diſtempers are generally accompanied with.

Sir John Pringle obſerves, that in the military hoſpitals, a putrid fever generally ariſes when they are crowded, and eſpecially if the weather be hot; that the ſame thing happens in crowded barracks, and in the holds of tranſport ſhips, when the hatches are ſhut; that, in ſhort, it attacks every place that is ill aired and kept dirty, i. e. full of ſteams from diſeaſed bodies; and that he has known the dyſentery and ſmall-pox changed into a putrid fever, by keeping a tent too cloſe ſhut up. As he takes notice of the heat of the weather favouring putrefaction, I could have wiſhed that he had alſo favoured the public with an account of that degree of heat which he found moſt conducive thereto. I could alſo wiſh to ſee the degree of heat determined in crowded barracks, jails, or the holds of tranſport ſhips, where this diſtemper generally begins, and afterward rages. I am fully perſuaded it would appear, that the heat is not nearly ſo great in any of theſe places, as in many others where no putrid diſtemper ever ariſes.

I remember, in the French priſon at Dundee, the jail fever made its firſt appearance in a ſmall low room with a ſtone floor, while the garrets, intolerably hot in the middle of ſummer, but better aired, were perfectly free; and though, in its progreſs through the jail, it afterward arrived at the garrets, it continued always moſt fatal in that room where it firſt began, which was the only low room in the houſe occupied by the priſoners; and as far as I can recollect, none ever took the fever but thoſe who ſlept in the ſmall rooms where the air was confined. Here we have a putrid diſeaſe, beginning in a cool damp place, ill aired, while ſeveral very hot dry ones, with a free circulation, remained perfectly free from it till the infection was repeatedly carried to them. And what Sir John Pringle mentions, of the ſmall-pox and dyſentery being changed into a putrid fever by keeping a tent too cloſely ſhut, appears to have happened much more from a ſtagnation of the air, than from any heat it could thereby acquire; for every one who has been encamped, knows that a tent can hardly become too warm, except while the ſun is beating upon it, and that it generally grows pretty cool during the night; and therefore the heat could hardly effect this tranſmutation; whereas the effluvia ariſing from the ſmall-pox, or from the fœtid dyſenteric excrement, by being confined and accumulated, might very eaſily do it.

Dr. Brockleſby, in his Account of the Diſeaſes of the Army, obſerves, that in the temporary hoſpital erected in the Iſle of Wight, which was very cold, from the ſlovenlineſs of the workmanſhip, fewer patients died than in the better quarters, though all were under the ſame regimen and medicines; and that in all places where fires were kept, though otherwiſe well aired, the patients died faſt, which did not happen in the places where no fires were. This obſervation ſeems to contradict ſome things that I have ſaid above; but upon conſidering the matter more fully, I am perſuaded the fact will be, that though, perhaps, no degree of heat may be able to produce putrefaction in a freely circulating air; yet if in this circulating air, putrid particles are already exiſting, it may render them more deleterious.

If great heat had a power of producing putrefaction in the animal fluids, there are ſeveral circumſtances in life which would make many people particularly obnoxious to it: as for inſtance, thoſe who work in glaſs-houſes, at large furnaces, &c. but we do not find that they are more liable to it than others. The inhabitants alſo of the hotter climates would be more ſubject to it than thoſe of the colder ones; yet I do not find, that in the Weſt Indies more people die of putrid fevers than in Britain; and Proſper Alpinus expreſsly denies that the plague, which annually viſits Egypt, is cauſed by the heat: *Ex caliditate* (ſays he) *aeris immodica peſtilentiam obortam fuiſſe nemo hactenus ibi vidit, obſervatum*

vatum vero eſt, ab inſigni aeris calore potius omne peſtiferum contagium extinctum eſſe. The plague indeed is not a native of our northern climates; and, on the other hand, it is no native of Egypt, of the Eaſt nor Weſt Indies, nor of many other climates, equally hot with theſe, from which it is often imported: other cauſes muſt therefore concur to produce this and every other putrid diſtemper beſides the heat of a climate, or of a room, or place where people are ſhut up. It is true, that the plague, and every other putrid diſeaſe, has been obſerved to decline, and at laſt totally to diſappear, during a long continuance of cold, dry, froſty weather; but we have no inſtance of moiſt foggy weather having the ſame effect, of whatever degree of cold it was: and therefore we may conclude, that the deſtroying of theſe putrid miaſmata, or rendering them inactive, is owing at leaſt to more cauſes than that of mere cold.

But to illuſtrate this ſtill further, let it be obſerved, that all the accounts I have ever met with of the origin of putrid malignant fevers, agree in this, that though they may attack individuals from a combination of particular circumſtances; yet they ſeldom or never become epidemic amongſt any number of perſons living in the open air, however warm this air may be; whereas, on the other hand, they ſeldom or never miſs to attack a multitude ſhut up in a cloſe place, let the natural heat of that place, or even of the atmoſphere, at the time the diſeaſe is produced, be ever ſo little. This reflection firſt gave birth to an opinion, which I am ſince ſtill more confirmed in; it is, that when a multitude are cloſely ſhut up together, the putrefaction which ariſes, does not depend ſo much for its cauſe on the heat of the place where they are confined, as on the ſeptic particles continually flying off from the lungs of every one preſent by expiration. For that the air expired from the lungs even of the moſt healthful, is replete with ſeptic particles, appears evident from ſeveral experiments;

of which I ſhall only mention the following. Six drachms of freſh mutton was divided into four equal parts, and each of theſe parts put into a ſmall phial with a little water: the upper part of three of theſe glaſſes was filled with air expired from the lungs of three different perſons, young, and in perfect health; and the upper part of the fourth glaſs was left full of the common atmoſpherical air: in this condition they were all corked, ſealed, and ſet together in a heat of about 84 degrees. The mutton in all the three glaſſes which contained the air that had been breathed, began to putrefy at leaſt ſeven hours ſooner than that piece contained in the glaſs with the common atmoſpherical air.

There can hardly be a ſtronger proof than this, that air expired from the lungs of the human ſpecies is a ſeptic; and if it becomes ſo very conſiderably ſo, by being only once breathed, what muſt it do in cloſe places, when breathed many hundred, nay, perhaps, many thouſands of times, and when entering every moment into a variety of different lungs, many of which are perhaps in a diſeaſed ſtate, whereby the whole atmoſphere of the place ſoon becomes an accumulation of highly putrid miaſmata? Hence it appears, that air often expired is of all others the ſtrongeſt pre-diſpoſing cauſe to putrefaction; and that the heat of a climate, of a jail, &c. can only operate as ſecondary and ſubordinate cauſes, by rendering the putrefactive contagion already exiſting, more active and virulent; but, as was obſerved before, can never call it forth into exiſtence.

It may, perhaps, be imagined, from my having given ſome hints, that I ſuſpected the degree of heat in putrid diſeaſes was below what is natural, as well as from ſome other obſervations I have made concerning the effects of it, that I am in theſe caſes an advocate for a warm regimen, in order to raiſe it to a proper ſtandard. I have no ſuch intention, as, ſhould I do ſo, it would be acting directly contrary to my own opinion. I have beſtowed a conſiderable ſhare of attention on this ſubject; but none of the obſervations reſulting from this attention, give me the ſmalleſt authority to conclude, that a warm regimen, or the heat of a place, will, in any manner, contribute to augment the conſtitutional heat; and far leſs to recover it when loſt.

We are not always coldeſt when our own ſenſations tell us ſo, nor warmeſt when we think we feel the greateſt heat. This is one proof of what I have juſt now mentioned; I ſhall add a few more.—After being long out in a cold winter day, when I imagined myſelf almoſt chilled to death; the mercury, in a ſmall pocket thermometer in one of my arm-pits, was almoſt two degrees higher than it was about three hours after, when I was ſitting and ſweating in a room with a very large fire. Nay, even in the cold fit of an ague, when the ſtrongeſt poſſible ſenſation of frigidity is preſent; it appears, by Dr. Home's experiments, that the degree of heat is often greater than what is natural to a healthful ſtate. The external atmoſpherical, or any other external heat, applied to the body in a free circulating air, ſeems to affect it very little, if not augmented many degrees above the conſtitutional heat of that body. It may ſeem a problem, that the heat of a man (and indeed of every other animal) does not riſe and fall in proportion to that of the aerial fluid with which he is ſurrounded. But however problematical it may be, it is certainly a fact: for let any man make the experiment, and he will find, that the mercury will generally riſe as high, if not higher, in the coldeſt weather of winter, as it will in the warmeſt of ſummer, by the application of a thermometer to any part of his ſkin. Nay, let a man carry a thermometer in his armpit in ſummer, whether he remain long in the ſhade, and long in the ſun, he ſhall find little, or perhaps no difference in the mercury. I ſhall only add further on this head, that I have been informed, that the real heat of the inhabitants of the north of Scotland, is, at a medium, fully as great as that of the inhabitants of the Weſt Indies, or any other of the warmeſt climates.

All theſe obſervations taken together, afford a ſtrong and convincing proof, that animal heat does not depend upon the degree of warmth which is applied to the animal *ab extra*; and ſome facts which I mentioned before, as well as ſome things I have juſt now taken notice of, ſeem to prove, that external cold had a greater power of augmenting animal heat, than even external heat itſelf: but ſhould this be found to be a fact, it muſt only take place when both the heat and cold are limited to certain degrees; for, beyond theſe, any creature may be roaſted or frozen to death. Be this matter as it will, it appears plain, that every animal is furniſhed with an internal principle of generating and preſerving its own heat; and that this principle is much more eaſily rouſed up and affected by internal,

ternal, than by external cauſes; for a large quantity of ſpirituous or vinous liquor, taken into the ſtomach, will, ſo far as I have obſerved, encreaſe the natural heat of a man's body much more than any tolerable degree of external warmth will: and in thoſe coldneſſes that generally attack the extremities before death, I do not remember ever to have ſeen any viſible benefit ariſe from the uſe of warm external applications; though I have ſeveral times ſeen warm generous cordials protract life longer than could naturally have been expected.

Having thus finiſhed what I had to obſerve concerning the effects of heat in producing or aſſiſting putrefaction, I ſhall now draw this inference from the whole, that *no reaſonable degree of heat applied to the body of any animal, has a power of producing or augmenting putrefaction in it, provided that the air it breathes be kept cool and circulating.* If the air is not kept cool, the ſeptic particles, as I obſerved before, continually flying off from the lungs and ſurface of the body of a human ſubject, may by the heat be rendered more deſtructive. If it is not kept circulating, they will be accumulated in greater numbers, be repeatedly taken back into the lungs, and thrown out ſtill more ſeptic at every expiration, till the whole air of the place become ſufficiently loaded with deſtructive particles, not only to accelerate the death of a perſon already infected, but alſo to infect thoſe who are in a ſound ſtate.

From what has been juſt now ſaid, I think it appears, that an atmoſphere confined, and rendered putrid by being often reſpired, is perhaps the only cauſe, why putrid diſeaſes ſo frequently attack all cloſe places where numbers of people are ſhut up; and therefore, all ſuch circumſtances ſhould be ſedulouſly avoided. When they become unavoidable, humanity ought to alleviate them as much as poſſible, by all the methods that can aſſiſt in making a circulation of that vital fluid ſo neceſſary to animal life. A very uſeful hint alſo concerning the management of patients in putrid diſeaſes, naturally ariſes from this fact: it is, that the curtains of the bed ſhould never be kept too much ſhut; for, if they are, the patient is thereby ſubjected to breathe the ſame air many times over, whereby he is ſoon involved in a putrid atmoſphere, which ought rather to be carried off by every poſſible endeavour. To effectuate which I ſhould not in the leaſt heſitate, not only to keep the curtains of the bed always open, but alſo to keep the doors and windows of the room frequently ſo. Nay, in order to carry off the putrid effluvia conſtantly ariſing from the lungs and body of the patient; it might even be adviſeable to place his bed in ſuch a manner, that a conſtant ſtream of air ſhould always be paſſing over him, which would effectually ſecure him from being again hurt by any of the putrid particles once thrown off by nature. This might perhaps be thought an attempt too daring, on account of the cold; and as it would be deviating conſiderably from the common road, would be ſtrongly objected againſt by that part of mankind who are led by cuſtom, and pay an implicit obedience to whatever has received the ſanction of time: but certain I am, it is founded upon reaſon; and I am conſcious, that no cold ariſing from it could be half ſo deſtructive as a highly putrid atmoſphere; eſpecially when we conſider, what was obſerved before, that *external heat or cold in a moderate degree, ſeems to have but very little power over the heat of an animal.*

As this ſubject of the effects of heat, has inſenſibly led me into an enquiry concerning the management of patients in putrid diſtempers; I ſhall beg leave to be indulged in a few more obſervations upon it, before I conclude.

As the breathing a cool freſh air, ſeems above all other things a *ſine quâ non*, directions to ſupply the patient plentifully with it, can never be too frequently or too ſtrongly inculcated: where this is impoſſible to be done, as in jails, the holds of ſhips, &c. every method we are capable of mentioning, ſhould be tried to correct and deſtroy the virulence of thoſe putrid particles which cannot poſſibly be diſlodged. Authors have from time to time contrived a variety of things for this valuable purpoſe; ſuch as burning aromatics in, or ſprinkling the room with, them; waſhing the room with vinegar, with ſpirits, &c.—It does not appear, however, upon the ſtricteſt enquiry, that theſe methods have been attended with any remarkable, nor indeed with any viſible ſucceſs. Their intention, indeed, is certainly a very rational one, viz. to impregnate the whole air of a room with antiſeptic matter, in ſuch a manner, that the patient may draw a good deal of it into his lungs at every inſpiration. But as their having hitherto done ſo little good, gives ground for a ſuſpicion, that they have either in this way not been intimately enough blended with the air, or not blended with it in a ſufficient quantity, I think other methods ought to have a fair trial alſo; eſpecially as there ſeem to be others, better calculated for rendering any antiſeptic matter more light and ſupportable by, and more diffuſible through, the air of a room.

It was obſerved before, towards the beginning of this Eſſay, that Dr. Macbride had ſweetened ſeveral pieces of putrid meat, by ſuſpending them in the ſteams ariſing from fermenting antiſeptics; and this, methinks, furniſhes us with a hint how to endeavour to correct the air of a confined place, and render it antiſeptic, where patients with putrid diſeaſes are; which is, by placing large quantities of fermenting antiſeptic mixtures in different parts of it. If this expedient ſhould not be found to anſwer, a ſtill farther trial may be made: let a large quantity of a decoction of bark, chamomile flowers, &c. when in the act of fermentation (into which ſtate it may eaſily be brought) be put by the patient's bed-ſide, and his head ſupported over it ſo as to breathe the ſteam as often and as long at a time as can be done. Should this method produce any good effect, it might very eaſily be improved by means of a machine contrived to convey the greateſt part of the ſteam ariſing from ſuch a mixture, into the patient's lungs.

In the beginning, at leaſt, of putrid diſeaſes, before the patient's ſtrength is much exhauſted, this might eaſily be tried: and I leave it to the judgment of thoſe converſant in the nature of antiſeptics, to determine whether it does not promiſe to be attended with advantages ſuperior to any of the methods mentioned above; only farther obſerving concerning it, that as theſe diſeaſes are always very alarming, and ſpeedily require every poſſible effort to be made againſt them, I think it the duty of every perſon, to whoſe care they are committed, to neglect no opportunity of trying every ſafe method of introducing as much antiſeptic matter into the blood as he can: and to this he will be the more readily diſpoſed, when he conſiders how unavailing moſt of the medicines hitherto preſcribed againſt putrid malignant diſtempers have been, and how little progreſs we have as yet made in curing them.

Though almoſt every medicine recommended by late authors for the cure of theſe diſeaſes, has been antiſeptic, and propoſed with a view to correct the putreſcency of the humours; yet, as far as I know, antiſeptics have only been given by the mouth, or injected into the inteſtines; both of which ways may be, and often are, hindered either by a vomiting or a purging. To theſe two ways, formerly practiſed, I have here humbly propoſed the addition of two others, that of introducing it thro' the ſkin, and by the lungs; neither of which, I flatter myſelf, can poſſibly be obſtructed by any accident that can happen.

I do not pretend here to lay down a plan for the management of malignant diſtempers through all their different ſtages and accidents; ſenſible that it would be a taſk far above my capacity, and that it is already, perhaps, as well executed as it poſſibly can be, by the accurate Dr. Huxham, and Sir John Pringle. I ſhall only make one obſervation more, which is, that in all the putrid diſeaſes I ever attended, ſo far as I can recollect, thoſe patients generally did beſt who had the leaſt ſenſible evacuations of any kind; that where profuſe ſweats came on, I do not think any one ever recovered; and but very few, where there were more than three or four ſtools in a day. This, I think, evidently points out to us, that theſe evacuations at leaſt ſhould be avoided; as alſo that the cure of theſe diſtempers is to be attempted almoſt ſolely by alteratives; for a contagion once received into the body, ſoon intimately blends itſelf with, and contaminates every part of it; and while the whole is thus contaminated, we may ſubſtract as many of the parts from it as we pleaſe, without leaving the remaining ones any better: while one ſingle infected particle is left, it will ſtill have a power of turning all the others into a like nature with itſelf; and were we to proceed intirely by the evacuatory method, ſo long as we left one particle, it would ſtill be an infected one.

I do not mean by this, that no evacuations ſhould ever be here attempted; where the primæ viæ are overloaded, it will certainly be highly neceſſary to relieve them; but whoever proceeds any farther without the moſt manifeſt indications, aſſuredly deviates widely from the path chalked out by nature, and endangers the life of his patient: the cure, therefore, can never be reaſonably attempted, by ſubſtracting a putrid from a putrid part; but by transforming the whole again into a ſound ſtate, by the introduction of ſuch antiſeptics into the body as we know from experience have a power of correcting and deſtroying putreſcency, and reſtoring the cementing principle, or bond of union, to matter, which keeps it from running into diſſolution.

As I think I have made it appear, by what has been ſaid above, that the degree of heat requiſite to make an antiſeptic bath penetrate the ſkin, cannot poſſibly do any harm in a putrid diſeaſe; and as I have plainly proved that diſſoluble antiſeptic ſalts, and even the particles of antiſeptic vegetables in a decoction, do penetrate the human ſkin in pretty large quantities; I ſhall now conclude the preſent Eſſay, with a view of the uſes that may be made of this diſcovery.

In the firſt place, it appears to me, that it would be an excellent means of preſerving the body from an epidemic peſtilential contagion; as alſo from the particular contagion of a jail, or any other confined place; as the body, by two or three times bathing, might be ſo well ſtored with antiſeptic particles, as to enable it to expel or deſtroy any ſeptic ones that might find entrance, either by the lungs or otherwiſe.

Secondly, Bathing in antiſeptics, as above recommended, and receiving the ſteams ariſing from them into the lungs, would certainly prove very powerful auxiliaries to their internal uſe; and by the conjoined force of theſe methods taken together, perhaps the progreſs of a diſeaſe might be ſtopped, which would prove too powerful for any one of them alone.

Thirdly, It affords at leaſt a probability of ſometimes ſaving a patient from the jaws of death, when internal remedies have failed, or when they cannot be retained in the ſtomach or inteſtines, in conſequence of which no benefit can be expected from them.

Fourthly, It points out an eaſy and ſafe method of curing the agues of children, who are too young to take ſo diſagreeable a medicine as the bark, or even of adults who have a natural antipathy to it; of whom there are not a few to be met with, though there are ſtill more who have acquired an averſion to it, and would ſubmit almoſt to any other method, however troubleſome, rather than be obliged to ſwallow it.

Theſe, I think, are the principal caſes in which the external application of antiſeptics will take place. The advantages which they have, when ſo applied, over the internal method, I have already hinted at: they are, *firſt*, a much greater quantity of the antiſeptic can be conveyed into the blood in this way, than when it is taken into the ſtomach. *Secondly*, Here they enter more immediately into the blood, than when obliged to go through the tedious courſe of chylification and ſanguification. *Thirdly*, The particles of an antiſeptic which enter into the blood in this way, are much leſs altered from their original nature, than thoſe which enter into it after they have undergone the action of the ſtomach, of chylification, and ſanguification. And, *laſtly*, No caſe or condition of the patient can prevent us from making this application; whereas ſeveral accidents may put it intirely out of our power to avail ourſelves of the other.

But neither from theſe very great advantages attending the uſe of antiſeptics externally applied, nor indeed from any thing that I have ſaid in this Eſſay, would I be underſtood to mean, that the internal uſe of ſuch medicines ought to be totally neglected. When nature is attacked by ſo potent an enemy as putrefaction, all the auxiliaries that can be brought to her aſſiſtance will be neceſſary; and therefore I would recommend both theſe methods joined together, not only at the beginning of the attack, but even when a perſon has been in an infected place, with this caution only, *always to let the primæ viæ be firſt cleanſed.*

In the greateſt part of the firſt of theſe experiments, I diſſolved nitre along with the bark. My reaſon was, becauſe at that time I knew nitre to be a ſtrong antiſeptic, and was ſure that it penetrated the ſkin; but was not then certain whether the bark would do ſo, as I had not made the experiments neceſſary to determine it. I am ſtill, however, of opinion that method may be uſeful, as thoſe antiſeptics may aſſiſt the operation of each other, and ſo be rendered more powerful.

ESSAY II.

On the DOSES and EFFECTS of MEDICINES.

IT had long been my opinion, that a great variety of things were retained in the materia medica, which were either altogether uſeleſs, or given in ſuch trifling doſes, that little or no benefit could reaſonably be expected from them. The fame of many of our preſent medicines has ariſen from accident; and ſtill more of them, perhaps, have been introduced into practice by the *ipſe dixit* of ſome celebrated perſon, who himſelf, with an aſſuming air of knowledge, had only aſſerted what he had learned by cuſtom, heard by tradition, or taken from the authority of another. In this manner, by much the greateſt part of the remedies at preſent made uſe of, have been handed down to us from our anceſtors, and through a long ſucceſſion of ages, their nature and virtues have eſcaped examination: Cuſtom has given them a ſanction, which Credulity has rendered ſtill more ſacred; and Indolence, conſidering it as the ſhorteſt and eaſieſt road to ſcience, to make uſe of the obſervations of others, has ſlothfully folded her hands, and declined the tedious way to knowledge by experiment and examination.

The end of every ſcience is, or ought to be, to render mankind more happy, either by obviating altogether, or alleviating, as much as is poſſible, thoſe evils to which human nature is incident; or by procuring thoſe benefits and pleaſures which the Author of our being has wiſely placed within the reach of an induſtrious application, either of our corporeal or mental powers. The ſcience of medicine evidently tends more than any other to the former of theſe valuable purpoſes; and therefore ought to be cultivated with the utmoſt care and aſſiduity; and when we conſider that it has been ſtudied almoſt time immemorial, by the moſt learned and ingenious men of every age; it is ſurpriſing that it ſhould ſtill appear in its infancy, and that the principles upon which it depends, ſhould ſtill be ſo vague and undetermined. To mention all the various reaſons that might be offered for this, would be deviating from my preſent purpoſe; I ſhall therefore only take notice of one of them, which is, the almoſt infinite number of remedies which have from time to time been introduced into the materia medica, and which have now ſwelled it to ſuch an immenſe bulk, that the longeſt life, and the largeſt experience, is incapable of ever becoming thoroughly acquainted with the virtues of one fourth part of the materials that compoſe it. Would it not therefore be much better for every practitioner to confine himſelf to the uſe of a few of the moſt valuable and approved remedies, by which means he would in time become tolerably ſenſible of what they were able to perform, than to launch out at once into the wide and unlimited field of natural productions, and rather preſcribe an endleſs variety of things at an uncertainty, than a few, whoſe operations he is certain he can depend upon.

Every young ſtudent who has read a number of medical books with a view of profiting by their inſtructions, though he has frequently met with very high encomiums on the virtues and effects of a vaſt variety of medicines, and thereby thinks himſelf provided with a ſufficient ſtock of materials to combat againſt every diſtemper that he may be called to; yet when he comes to try them, will (I am ſorry to ſay it) be too often mortified to diſcover that he can neither find them poſſeſſed of any virtue, nor productive of any effect. Frequent diſappointments of this nature, and a deſire of having the doſes and effects of the medicines moſt commonly made uſe of, more properly aſcertained than they are at preſent, were the motives which induced me to make the following experiments; which I intended to have proſecuted much farther, had not one of them almoſt proved fatal to me, and ſome of the others given me ſo much uneaſineſs, that, from motives of ſelf-preſervation, I was obliged to deſiſt. However, what will appear from the following facts, will be ſufficient to ſhew how inſignificant ſome things are, which have long retained a very high character; and how much others have been trifled with, by being given in very ſmall quantities, which in larger doſes might have anſwered the moſt valuable purpoſes.

As caſtor has long been eſteemed, by the general conſent of practitioners, a very powerful antiſpaſmodic, and cordial reſtorative; and as it has been univerſally received an ingredient in the greateſt part of antihyſteric medicines, I reſolved to try whether I could diſcover by its effects, how far it would anſwer thoſe purpoſes for which it is ſo often preſcribed.

EXPERIMENT I.

I took a bolus of ten grains of caſtor, made with a little ſyrup of ſugar. A thermometer had been applied to the pit of my ſtomach half an hour before I took it, during which time the mercury had riſen to 99 degrees; it was kept there for two hours more, but the mercury never aroſe any higher. My pulſe, before the experiment, had beat 71 ſtrokes in a minute: it continued for ſome hours after, beating ſometimes 70, and ſometimes 71. I felt no other effect from the bolus than a few diſagreeable eructations.

EXPERIMENT II.

The following day I took a bolus of half a drachm of caſtor. This neither produced any alteration on the mercury in the thermometer at my ſtomach, nor on the number of my pulſations in a minute; the only effect it had was the eructations, which were pretty much the ſame as in the laſt experiment.

EXPERIMENT III.

Two days after this, I took one drachm of caſtor. The mercury in the thermometer at my ſtomach, which before taking it had ſtood at 91½, was in an hour after riſen to 92½, where it continued till the thermometer was removed. My pulſe was not in the leaſt altered, nor was I ſenſible of any other effect than the eructations, which were neither ſo frequent, nor ſo diſagreeable as in the former experiments.

EXPERIMENT IV.

The two following days, I took two more boluſes of caſtor; the firſt containing one drachm and a half, and the ſecond two drachms. But I could neither diſcover by the thermometer, that they made the ſmalleſt difference on the natural heat of my body; nor by the number of pulſations in a minute, that they had any way affected the circulation. I ſhould not have been ſenſible that I had taken them from any other effect than the eructations, which indeed were but few and trifling.

Whatever is the *modus operandi* of alterative medicines, or what power they may have to change the nature of the blood and other fluids, without affecting us ſenſibly while they are doing it, I ſhall not pretend to determine. But ſurely a medicine which has been ſo much eſteemed as a cordial, and ſo often given to raiſe and exhilarate the depreſſed ſpirits, ought to do ſomething to convince a perſon that he has taken it. The uſe of wine, a generous meal, moderate exerciſe, and chearful company, all tend to diſſipate what is called lowneſs of ſpirits: but they operate in a viſible manner; they augment the natural heat and force of the circulation; whereas caſtor, as appears from theſe experiments, does neither; for I felt no manner of effect from any of theſe doſes, though the laſt of them was at leaſt four times as large as any I had ever known to be given. If there is therefore any virtue in this drug, anſwerable to the intentions for which it is uſually preſcribed, it muſt be given in much larger doſes than has hitherto been done; and ſince two drachms of it could be taken not only with impunity, but without operating in any ſenſible manner, how trifling and inſignificant muſt fifty or ſixty drops of the tincture be, when they do not contain above one grain of it.

Neumann and Sthal, as far as I know, were the firſt who doubted the virtues of caſtor. Several eminent practitioners of late have followed their example, and ſeldom preſcribed it but to pleaſe their patients, with many of whom it ſtill retains a celebrated name, on account of its high price, and the large doſe that muſt be neceſſary to produce any valuable effect. I think it but ill deſerving of a place in the preſent catalogue of medicines; as a ſmall doſe of it, I am ſure, is uſeleſs, and a doſe of it large enough to be uſeful (if any doſe of it can be ſo) will be much too dear to be obtained by the generality of mankind, and much too tempting to eſcape ſophiſtication.

I ſhall conclude this remark by obſerving, that caſtor has been much eſteemed for its virtues as an antiſpaſmodic. The experiments I made with it gave me no opportunity of determining this; but, from the moſt accurate obſervations I have been able to make, and from the accounts I have received from others, no benefit has ever perceptibly ariſen from the uſe of it in ſpaſmodic caſes.

EXPERIMENTS

WITH

SAFFRON.

SAFFRON, as well as many other medicines whoſe operations have never been minutely inquired into, has not wanted a number of authors who have extolled its virtues, and celebrated its praiſes in a manner perhaps a little extravagant. An inſtance that was mentioned to me of a lunatic, who ſwallowed a very large quantity of it without being hurt, gave me the firſt hint of inquiring into its efficacy, by the following experiments:

EXPERIMENT I.

Ten grains of ſaffron, made into a paſte with a little bread, was taken in the morning on an empty ſtomach. It made no alteration on the mercury in the thermometer at my ſtomach: it did not affect my pulſe, nor operate in any manner that I could be ſenſible of.

EXPERIMENT II.

The next day I took one ſcruple of ſaffron, which did not alter the height of the mercury, though my pulſe ſoon after was more frequent by two or three ſtrokes in a minute. This I imagine was accidental, as I felt no effect from my doſe.

EXPERIMENT III.

As none of the doſes in the laſt experiments had given me any kind of ſenſation by which I could diſcover their operation, I ſome days after took two ſcruples of it. An hour afterwards I found the mercury in the thermometer at my ſtomach riſen one degree: upon this I expected that my pulſe would be riſen alſo, but was very much ſurpriſed to find it fallen from 72 to 66; and ſtill more to find that it continued about 66 and 67 all the reſt of that day.

EXPERIMENT IV.

Some days after this I took four ſcruples of ſaffron. This had no manner of effect, either on the mercury in the thermometer at my ſtomach, or on my pulſe; ſo that I concluded the remarkable diminiſhing of it in the laſt experiment had not been owing to any effect of the ſaffron, but to ſome other cauſe. Though this doſe was very much larger than any that are commonly given, yet I neither was ſenſible of the ſmalleſt effect from it, nor from any of the others; and therefore I deſiſted from taking any more, being fully perſuaded that, if the doſe of ſaffron is to ariſe above a few ſcruples, there are few patients who will ever be prevailed on to take it, as it has a nauſeous, diſagreeable taſte, and can hardly be diſguiſed by mixing it with any other preparation.

Galen is of opinion, that the liberal uſe of ſaffron either takes away reaſon, or procures death; and Boerhaave has claſſed it among the narcotic poiſons. But, with all deference to the authorities of two ſuch illuſtrious authors, as well as to that of ſeveral others, whoſe opinions have been nearly the ſame, I cannot help thinking that it is a medicine (if it deſerves that name) juſt as innocent and as uſeleſs as any in all the materia medica: at leaſt, thus far I am certain, that if any good, or indeed any ill, can be done with it, the doſes muſt be made infinitely larger than any that the preſent practice allows of.—While I was taking this drug, I expected it would have perhaps tinged my urine, the colour of which I examined carefully from time to time, without being able to diſcover the leaſt alteration in it. I then tried it by ſteeping in it pieces of clean linen rags, and of white paper; but it left no dye upon any of them; a plain proof that there was none of the ſaffron in it. I next examined the linen which I had wore, but could find nothing like the dye of ſaffron upon it, nor any alteration from the colour that it uſed to be when I threw it off. The excrement, however, that I paſſed, was very ſtrongly tinged with it. From all which obſervations it appears evident, that it does not enter into the blood; for if it did, ſome of it muſt be in the urine; and if any of it were there, it would certainly tinge a bit of linen or paper. What ſeems moſt probable is, that it paſſes entirely off through the primæ viæ; and therefore is not very well qualified to do any of that good or evil which ſome have aſcribed to it.

EXPERIMENTS

WITH

NITRE.

THE greateſt part of the neutral ſalts ſeem, from what obſervations have hitherto been made upon them, to be poſſeſſed of very conſiderable ſudorific and diuretic qualities. Among the moſt valuable of this claſs is nitre, not only as a ſudorific and diuretic, but alſo as a powerful cooler and a ſtrong antiſeptic. Several other virtues have been attributed to it, of which we are not ſo certain; however, as theſe already mentioned are enough to render it very valuable, I thought a few experiments with it, in order to determine to what quantity it may be given, and what its effects are on the human body, would not be unacceptable to the public.

Having made a number of experiments on the frigorific power of nitre, when diſſolved in fluids, I conſtantly obſerved, that on putting a thermometer into any ſort of them, and afterwards throwing in powder of this ſalt, the mercury fell almoſt immediately to the loweſt degree that it would go in that ſolution; and, in a minute or two after, began gradually to ariſe again, till it came to the ſame height at which it had been before the nitre was put in. As nitre effervеſces but very little when mixed with any liquid, I ſuſpected that the cold produced by it was not owing to that cauſe, but to ſome quality in the nitre itſelf, which the external air, perhaps, ſeized on and carried away, when the ſolution was expoſed to it.

EXPERIMENT I.

In order to ſatisfy myſelf concerning this matter, I took two four-ounce phials, and having filled them nearly full of water from the ſame bottle, I put into each of them two drachms of powdered nitre. One of them I corked and ſealed with wax, and leaving the other without a cork, ſet them both together in a cool place. After they had ſtood two hours, I poured the contents of that which had been expoſed to the air into a tea-cup, and put the thermometer into it. In about a minute, the mercury ſunk five degrees, but would go no lower. I then poured the ſolution that had been corked and ſealed, into another cup, and having raiſed the mercury the five degrees it had fallen in its laſt immerſion, put the thermometer into it alſo; but the mercury in this only ſunk three degrees.

The next day I repeated the ſame experiment. In the ſolution which had been corked, the mercury fell only two degrees; whereas in that which had not been corked, it fell almoſt five.

EXPERIMENT II.

Two four-once phials were filled with *ſpiritus mindereri*, and ſet together for one night; when in one of them, which had been corked, the fall of the mercury was hardly perceptible; in the other, it fell two degrees.

EXPERIMENT III.

I took two ſmall phials, which contained each two ounces, and filled them with compound horſe-radiſh water; one of them I corked immediately, and left the other expoſed to the air: after they had ſtood three hours in the ſame place where the water was uſually kept, I put the thermometer into a ſmall tea-cup, and poured the liquor that had been expoſed to the air upon the ball of it: the mercury (when the liquor was all poured out) had fallen two degrees, but would go no lower. I then changed the thermometer into another cup, and poured on it the liquor from which the external air had been excluded: as ſoon as it touched the ball of the thermometer, the mercury began to ariſe; and when the whole of it was poured out, it had riſen nearly the two degrees which it had ſunk by the effuſion of the former liquor, but would riſe no higher.

EXPERIMENT IV.

This ſudden riſe of the mercury, which had not happened in any of the former experiments, ſurpriſed me ſo much, that to ſatisfy myſelf further concerning it, I again returned each quantity of liquor into the ſame glaſs from which it had been taken; corked the one that had been left uncorked before, and *vice verſa*. In this manner, and in the ſame place where they had been in the laſt experiment, they ſtood all night. Early next morning, I put the thermometer into a cup, and poured on it the liquor that had formerly been corked, and was now expoſed to the air: by this the mercury fell two degrees. I then changed the thermometer into another cup, and poured on it the liquor that had formerly been expoſed to the air, and was now corked: the mercury, while this was pouring on it, aroſe almoſt the two degrees it had fallen by the other liquor.

From this it appears, that the relative heat of two equal quantities of the ſame liquor may be altered, and any one of them made hotter or colder than the other, by excluding it from, or expoſing it to, the air.

EXPERIMENT V.

I filled the ſame phials with camphorated ſpirit of wine, taken from a bottle that had always ſtood in a north room, and expoſed them for two hours to the ſun in a ſouth window; I then put the thermometer into a cup, and poured the liquor out of the uncorked glaſs upon it; the mercury aroſe four degrees, but would go no higher. I next changed the thermometer into another cup, and poured the contents of the corked phial upon it, by which the mercury aroſe two degrees more.

EXPERIMENT VI.

I filled the ſame glaſſes again with the horſe-radiſh water, and left them both in the ſame place expoſed to the air: when they had ſtood two hours, I examined them, and found them both exactly of the ſame degree of heat. I then corked one of them, and left them there three hours longer; and on examining them again, found that the liquor in the corked phial was one degree and a half warmer than that in the uncorked one.

EXPERIMENT VII.

Two glaſſes, full of pure water, the one corked, and the other uncorked, ſtood together three hours, in the ſame place where the quantity of water from which they were taken had ſtood before. On examining them, the water in the corked phial was almoſt one degree warmer than that in the other; and on comparing the heat of the water in the uncorked phial, with that of the water from which it was taken, they were exactly equal; but on comparing the other, it had acquired almoſt one degree of heat greater than the original quantity, during the time it had been ſhut up and ſeparated from it; for which acquiſition, no other cauſe could be aſſigned than its excluſion from the external air.

Theſe experiments, together with a variety of others whoſe effects were nearly ſimilar, inſtead of confirming my conjecture, that the air carried away the coldneſs from diſſolved nitre, plainly demonſtrated the contrary; and not only diſcovered, but confirmed a fact which I had never ſo much as thought of, viz. that *a given quantity of any fluid, excluded from all communication with the external air, ſoon becomes warmer than any other given quantity of the ſame fluid, left expoſed to it.*

From this I was led to conjecture, that not only fluids, but perhaps all, or the greateſt part of other bodies, may acquire heat, when excluded from the circulating air; and even that the air itſelf may become warmer when cloſely ſhut up, than when at liberty to communicate with the external atmoſphere. This conjecture ſeems to be confirmed by the following experiments.

EXPERIMENT VIII.

Two thermometers, graduated exactly to each other, were hung in a room; one upon the inſide of a cloſet door, and the other on the outſide of it: the mercury was always one degree higher in the thermometer on the inſide, than in that on the outſide; but when the cloſet door was left for ſome time open, they exactly agreed.

EXPERIMENT IX.

One of theſe thermometers was put into a ſmall partition of a writing-deſk, which was then locked, and the other laid on the outſide of it. The mercury in that which was ſhut up, ſtood always one degree and a half higher than in the other.

EXPERIMENT X.

A thermometer was put into an empty phial, and the mouth of the phial well luted; ſo that there could be no communication between the incloſed and external air: in this ſituation it ſtood a night, and on taking away the luting in the morning, almoſt as ſoon as the external air ruſhed in, the mercury ſunk one degree.

From theſe experiments I was induced to think, that there is a ſtronger refrigerating principle in the circulating, than in the ſtagnant air; it was therefore natural to infer, that this principle (if it really exiſted) would be increaſed in proportion to the compreſſion of the air, and velocity of its motion; but by blowing forcibly on the ball of a thermometer with a pair of hand-bellows, the mercury always, in a minute or two, roſe more than one degree; and in ſome trials it roſe ſeven or eight, and as conſtantly fell three, four, or more, when put into a window juſt lifted up far enough to admit a very ſtrong draught of air.

The reaſon of theſe ſo very different phenomena, in circumſtances ſo ſimilar, I ſhall not attempt to explain, but reſume the experiments with nitre, which was the original intention of this Eſſay.

EXPERIMENT XI.

I mentioned before, that in making ſome experiments with nitre, I had conſtantly obſerved, that it poſſeſſed a very great power of producing artificial cold, when diſſolved in any fluid; which led me to endeavour to diſcover, whether the internal uſe of it would alter the conſtitutional heat of my body. For this purpoſe, I applied a thermometer to the pit of my ſtomach, and the higheſt degree to which the mercury would riſe, was 98, my pulſe beating 72 ſtrokes in a minute. I then took a drachm of nitre diſſolved in an ounce of water; two minutes after this, my pulſations were reduced from 72 to 64; four minutes after, they were as low as 62; and from that time they began gradually to increaſe, till at the end of ten minutes they were at 70, and ſoon after at 72, the exact number at which they were before I took the draught. About 20 minutes after I had taken the nitre, on looking at the thermometer, the mercury had ariſen from 98 to 99½; and in 20 minutes more, it was fallen again to 98, and my pulſe ſtill continued to beat 72: this was exactly, in every reſpect, the ſtate in which I was before I took it.

As the riſing and falling of the mercury in all the ſubſequent trials was extremely irregular, I ſhall leave out of my narrative of the following experiments, the obſervations I made on it, and lay it down as a poſtulatum, that *whatever power nitre may have of cooling the body, it does not exert it in any perceptible manner on its external parts.*

EXPERIMENT XII.

About an hour after I had taken the firſt draught, I took a ſecond. My pulſe beat 70 before I took it, but in one minute after, no more than 60, though it ſoon became quicker; ſo as, at the end of ten minutes, to beat 68, and, in a few minutes more, 70. As ſoon as I had taken it, I felt a chillneſs over all my body, but more particularly at my ſtomach, which continued for about 20 minutes to give me a good deal of uneaſineſs. It then began to decreaſe, and in little more than half an hour was intirely gone off.

EXPERIMENT XIII.

The next day I repeated the ſame experiment. Before I took the doſe my pulſe beat 64; the ſecond minute after, the ſtrokes were reduced to 60; the fifth minute after, they were at 63; and ſoon came to 64, as before I took it.

EXPERIMENT XIV.

As the nitre had been ſo ſtrong and diſagreeable to my ſtomach when ſo little diluted, the day following I took a drachm of it diſſolved in two ounces of water. Before I took it my pulſe beat 73; the ſecond minute after, it fell to 66; the fourth minute after, it aroſe to 69; and from that time became ſtill more frequent; till, at the end of nine minutes, it had recovered its uſual ſtrength, and was at 73.

EXPERIMENT XV.

Twenty minutes after this doſe, I took a drachm and a half of nitre, diſſolved in three ounces of water. After two minutes my pulſe was weak, fluttering, and unequal, and beat about 70 in a minute.

Soon after I felt a painful ſenſation at the upper orifice of my ſtomach; and, on ariſing from my chair, it was with ſome difficulty that I walked through the room. I then returned to the chair, and felt my pulſe again. It was now become ſo quick, fluttering, and irregular, and my head was ſo giddy, that I could not exactly number the ſtrokes it beat, though, as near as I could judge, they were between 96 and 100. In about an hour, every one of theſe diſagreeable ſymptoms began to abate, and continued ſlowly decreaſing all that day. The next morning, when I got out of bed, they were intirely gone off*.

EXPE-

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* Soon after this experiment, on Sunday the eighth of September, 1765, I was called to the wife of a grocer in this city, who, intending to take a doſe of *ſal Glaub.* ſent her maid into her ſhop to bring a handful of it, directing her to the drawer where it lay. The maid miſtook the drawer, and, inſtead of the *ſal Glauber.* brought a handful of nitre, diſſolved it in warm water, and gave it to her miſtreſs, who, in order to avoid as much as poſſible the diſagreeable taſte of the *ſal Glaub.* (which ſhe

EXPERIMENT XVI.

I had taken every one of the preceding doſes as ſoon as the nitre was diſſolved; and

ſhe ſuppoſed it to be) ſwallowed the whole draught with that precipitation which is natural in theſe caſes; but was ſurpriſed to find a ſtrength and pungency in it which ſhe had never diſcovered before in taking ſalts; inſomuch that, to uſe her own phraſe, it had like to have choaked her. Immediately after ſhe had taken it, a very ſevere pain aroſe in her ſtomach; upon which ſhe ſuſpected that ſhe had got ſomething elſe inſtead of the ſalts ſhe intended to take. She therefore deſired the maid to ſhew her the drawer from whence ſhe had taken them, which was the drawer where the nitre then lay.

While they were making this diſcovery, ſhe ſickened, and threw up a few mouthfuls, which taſted very ſtrongly of the ſalt. From the very moment ſhe had taken it, ſhe began to ſwell, and continued to increaſe in ſo ſurpriſing a manner, that at the end of this vomiting, though not above three or four minutes had elapſed ſince ſhe had taken the doſe, the lace of her ſtays was ready to burſt aſunder; and it was with much difficulty they could be got off ſoon enough to allow room for the increaſing bulk of her body. Her neck too was affected in the ſame manner, and ſo very much enlarged, that her necklace had almoſt ſtrangled her while the aſſiſtants were and having by them fully ſatisfied myſelf that its effects, when ſo taken, were very evident

were taking it off; nay, even her petticoats and garters were obliged to be looſed, ſo univerſally did the ſwelling extend itſelf. All this happened in the ſpace of ſix or ſeven minutes; nor was it more than ten from her taking the doſe when I ſaw her. As ſoon as I had diſcovered what was the occaſion of her complaint, I immediately ordered her a vomit of ipecacoana; and, the moment after ſhe had ſwallowed it, gave her large draughts of oil and warm water. By the aſſiſtance of theſe, ſhe ſoon vomited pretty freely, and in proportion as the vomiting increaſed, the pain and ſwelling decreaſed; ſo that, after five or ſix plentiful evacuations, they were both greatly abated. Having now recovered a little from the panic into which ſhe had been thrown, ſhe was extremely ſolicitous to have the remains of the nitre carried off, and therefore propoſed to drink ſome of the *ſal Glauberi*, in order to purge away any part of it that might be got into her inteſtines. I complied with her requeſt, in hopes that the ſalts would make her vomit more freely than ſhe had hitherto done: which happened accordingly; for ſhe had no ſooner drank a large draught of them, than ſhe threw them all up again, together with ſome of the oil and water which had remained in her ſtomach. Immediately after this, ſhe had a very profuſe looſe ſtool, accompanied with a little griping; after which ſhe was put to bed, where, in about half an hour, ſhe had evident and conſiderable, I now proceeded to try whether they would be the ſame [illegible] when [illegible] had an abortion, having been two months pregnant. After the fœtus was come away, ſhe began to evacucuate blood *per vaginam* & *per anum* along with every looſe ſtool, of which ſhe had a great many that day. On Monday, this evacuation, together with the flooding, were ſomething leſſened; but on Tueſday they returned with greater violence than ever, and what ſhe then paſſed by ſtool ſeemed to be nothing but the villous coat of the inteſtines mixed with blood. On this account I ordered her ſome mucilaginous medicines, with opium; by the help of which theſe ſymptoms were much abated on Wedneſday, and on Thurſday night were almoſt intirely gone off. Beſides the ſwelling and pain in her ſtomach, which had ſeized her immediately after taking the nitre, ſhe had been attacked alſo with violent pains over her whole body, but more particularly in the ſmall of her back: theſe, however, did not continue very long, being almoſt intirely gone on the Monday, though ſhe had ſome ſlight returns of them after. On Sunday, about twelve o'clock, her head began to be affected, and ſoon after grew ſo giddy that ſhe could hardly ſit up in the bed: this was accompanied with a ſinging in her ears; an univerſal tremor over her body; and an exceſſive chillneſs, which neither warm liquor, nor all the bedcloaths they could heap over her could remove. The

giddineſs

when it was taken after it had remained ſome time in a fluid ſtate. For this purpoſe I diſſolved one drachm and a half of it in three ounces of water, which I left twelve hours expoſed to the air, and then ſwallowed. Immediately before I took it, my pulſe beat 64; the ſecond minute after, it beat the ſame; the fourth minute after, it beat 59; and from that time began to increaſe as in the former experiments,

giddineſs and ſinging in her ears laſted till Monday afternoon, the tremor ſtill longer, and did not intirely diſappear till Wedneſday. But the coldneſs, which had been exceſſive all the Sunday afternoon, went off ſome time after her huſband went to bed to her.

Her throat was a good deal excoriated by the acrimony of the nitre, and it is very probable that her ſtomach had ſuffered in the ſame manner; for ſhe could not till Thurſday, ſwallow any thing that had the ſmalleſt degree of pungency, without ſuffering very ſeverely, both during the time it paſſed her throat, and for ſome time after it got into her ſtomach; though at the ſame time ſhe could uſe mild and mucilaginous things, ſuch as linſeed tea, or ſweet milk, with very little pain either in her throat or ſtomach.

ments, till it came to the ſtandard at which it had been before I took the nitre.

On comparing this experiment with the former ones, the difference appears very conſiderable; for the effects of one drachm newly diſſolved, were much greater, and more evident than the effects of a drachm and a half which had remained long in a fluid ſtate.

EXPERIMENT XVII.

Having now pretty well aſcertained the quantity of nitre I could bear at one doſe, and alſo diſcovered that its effects were much ſtronger when given newly diſſolved, than when it had remained long in a fluid ſtate, I next reſolved to try how often I could bear theſe doſes to be repeated. For this purpoſe I diſſolved ſix drachms of it in a quart of water, which I began to drink early in the morning; and by taking ſmall draughts of it as often as I had convenience, I finiſhed the whole at eight o'clock that night, without feeling any uneaſineſs from it, or being ſenſible of its having operated any other way than by urine.

EXPERIMENT XVIII.

Two days after, I diſſolved one ounce of nitre in the ſame quantity of water, and drank it in the ſame time; it gave me no uneaſineſs, nor had any ſenſible effect.

EXPERIMENT XIX.

Some days after this, I diſſolved one ounce and a half of nitre in three pounds of water, and took a draught of it every hour, except when in bed: the whole was drank in twenty-four hours. After four or five draughts, I felt a ſlight chillineſs at my ſtomach every time I took it; but this generally went off before the time of taking the next draught, and on that account gave me but little pain.

EXPERIMENT XX.

I now reſolved to try what would be the effect of the ſame quantity of nitre, when every different doſe was taken immediately on its being diſſolved. For this purpoſe, I divided one ounce of it into eight equal parts, and took one of theſe parts, diſſolved in four ounces of water, every ninety minutes. The weather was at this time very warm, and therefore the firſt three or four doſes cooled and refreſhed me; the fifth and ſixth, however, gave me a chillineſs and pain in my ſtomach; the ſeventh and eighth increaſed theſe ſharp ſtinging pains, not only in my ſtomach, but through my whole body; which were ſo violent, that for fifteen minutes after each doſe, I could not breathe without feeling a very acute pain every inſpiration.

EXPERIMENT XXI.

As I had been able to take one ounce and a half of nitre with very little inconvenience when it had been long diſſolved, I reſolved to make one more effort to try if I could manage the ſame quantity, when every doſe was taken immediately after being diſſolved. I therefore prepared eight powders of a dram and a half each, with a deſign to take one of them every ninety minutes, as in the laſt experiment: the ſecond doſe gave me a chillineſs at my ſtomach; the third gave me ſome of the above-mentioned pains; and the fourth increaſed them to ſuch a violent degree, that I was obliged to deſiſt from taking any more.

From ſome of the former of theſe experiments, it appears evident, that nitre has a power of almoſt inſtantly retarding the velocity of the circulation, and of ſurpriſingly diminiſhing the number of pulſations. Whether any real medical advantage may be derived from this, I ſhall not poſitively affirm; though I think it is very poſſible, that in caſes where the momentum of the blood is ſo great, from any ſudden cauſe, that the veſſels are in danger of being ruptured, a large doſe of nitre inſtantly given, might throw a ſort of damp upon the vital flame, and obviate that misfortune till the patient could be aſſiſted by bleeding and other remedies. And I would further infer, from the chillineſs produced by large doſes of it in my ſtomach, and the refreſhing coolneſs it diffuſed over me in the warm weather, that if given immediately after being diſſolved, it would prove a highly uſeful medicine in all ardent inflammatory diſtempers, where great thirſt, a dry tongue, and a ſtrong pulſe, indicate the uſe of cooling antiphlogiſtic remedies. This inference is not founded on mere ſpeculation and theory, but on experience and obſervation alſo; for as ſome of theſe experiments which diſcovered its inſtantaneous operation on the circulation, were made near three years ago, I have ſince then had ſeveral opportunities of trying it in inflammatory caſes, and have ordered it to the quantity of two ſcruples every hour, or every hour and a half, taking care that every different doſe ſhould be given newly diſſolved. In this way I have generally ſeen it ſit very eaſy on the ſtomach; often procure great remiſſion of the ſymptoms; and almoſt always either work off by a plentiful diſcharge of ſweat, or urine, according as the patient took along with it warm or cold drink.

I would by no means inſinuate that this is a new practice; for the illuſtrious Mr. Boyle, in his experiments on the redintegration of nitre, calls it one of the coldeſt bodies in the world, and adds, that "on this account phyſicians and chymiſts were wont to give it to allay the inward exæſtuations of the blood." All, therefore, that is uncommon in the uſe of nitre in febrile caſes, is the giving it immediately after the ſalt is diſſolved; which I was firſt induced to do, by obſerving, that a ſolution of it very ſoon loſt that coldneſs of which it was at firſt poſſeſſed, whether it was kept ſhut up, or in the open air. The trials I afterward made with it on myſelf ſhew, that when it was long kept in a fluid ſtate, it loſt, in a great meaſure, its power of affecting my body alſo. This will appear by comparing Experiments XI, XII, XIII, XIV, and XV, with Experiment XVI; and by comparing Experiments XVI, XVII, XVIII, and XIX, with Experiments XX, and XXI, will be further illuſtrated and confirmed.

Whether nitre will communicate cold to the body of a living animal, in the ſame manner as it does to water when diſſolved in it, is what I could not diſcover by the thermometer. The ſenſations, however, which I felt, after taking large doſes of it, induce me to think that it does; and the extraordinary cold felt by the lady in the caſe I related, together with the remarkable ſinking of my pulſe, and the effects of it in inflammatory diſtempers, all ſtrongly corroborate this opinion. If I had ſeen the lady during the time her cold fit laſted, I ſhould have had the beſt opportunity that perhaps has ever offered, of determining, by the application of the thermometer, whether its frigorific power reached to the external parts of the body; but, unfortunately, I knew nothing of this complaint till it was intirely over. On mentioning her caſe to Dr. Alexander Monro, profeſſor of anatomy, I was by him favoured with a ſight of Dr. Clerk's account of the caſes of three journeymen ſhoemakers, who all at the ſame time had taken large doſes of nitre, two of them two ounces each, and the third an ounce and a half. They were all ſeized immediately with a burning heat at their ſtomachs, accompanied with vomiting, which are all the ſymptoms mentioned. If this was literally true, it would overturn the theory of nitre acting as a cooler: but I imagine what they called a burning heat was not ſo much a real ſenſation of heat, as of pain occaſioned by the pungency of the nitre; and my reaſon for this opinion is, becauſe, on examining the common people of this country, I have generally found that they deſcribe almoſt every complaint of the ſtomach by the name of a burning heat. As little regard is therefore to be paid to their definition of any ſenſation, I think this ſymptom, to which they gave the name of heat, is by no means a proof that it really was ſo; or that nitre has any power to augment the conſtitutional warmth of any animal, as we ſee it ſo evidently poſſeſſed of a quite contrary power, when mixed with any fluid out of the body.

When I began theſe trials, I expected that the effects of nitre would have been ſo viſible, as to have enabled me to determine to what degree of cold it was capable of reducing my body below its uſual ſtandard. But though I have been diſappointed in this, perhaps future experiments, and more accurate obſervations, may ſtill diſcover it: and though I have not been able to throw that light which I wiſhed and expected on this quality of it, yet I have certainly demonſtrated that a much larger quantity of it may be taken, than any perſon that I know of had ever done before me; and that not only by the experiments on myſelf, but, ſince they were made, by giving it in nearly the ſame doſes to others, without having ever met with any complaint of conſequence from this liberal uſe of it *; ſo that we may eaſily ſee how trifling and inſignificant the common method is, of giving only a few grains at a doſe, and repeating theſe doſes at ſuch long intervals, as perhaps not to take above three or four of them in a day. We may alſo learn from theſe experiments, that when it is given as a cooler, the Decoct. Nitroſ. of the Edinburgh Diſpenſatory, or any other preparation of it, where it remains long in a fluid ſtate, are very unfit methods of exhibiting it, as they intirely diveſt it of that quality which was the ſole intention of preſcribing it.

After a number of repeated trials had thoroughly convinced me, that large doſes

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* When theſe experiments were made, I had not ſeen Dr. Brocklesby's book; but have read it ſince, and find that he uſed to give ʒx of it in twenty-four hours with great ſucceſs; which I am perſuaded would ſtill have been greater, had he given it always newly diſſolved.

of this ſalt had an almoſt immediate power of diminiſhing the number of my pulſations in a minute, I imagined that this was owing to its cold leſſening the irritability of the heart, and therefore concluded that any cold body received into the ſtomach would, in ſome degree, have the ſame effect. Upon trial, I found this concluſion to be juſt: for large draughts of very cold water, haſtily drank, always leſſened the number of pulſations in a minute, three, four, or five, and ſometimes more; which ſhews the abſurdity of condemning cold water in fevers, and at the ſame time allowing cold draughts, medicated with nitre, to be given; though it appears that they both act in the ſame manner, only the latter is much more powerful than the former, and therefore, on the hypotheſes by which cold water is forbid, ſhould do more miſchief.

Was I to endeavour to give an account of all the virtues which have from time to time been aſcribed to nitre, I ſhould ſwell this Eſſay much beyond my intention. I ſhall therefore refer the reader to *Hoffman de ſalium mediorum, & de præſtantiſſimâ nitri virtute,* and to *Sthal de uſu nitri medico,* where ſeveral curious obſervations on its virtues and effects are mentioned. Dr. Lewis, a later writer of no ſmall credit, reckons, that it often gives relief in ſtranguries and heat of urine, proceeding either from a ſimple or a venereal taint; and indeed the greateſt part of practitioners have always given, and ſtill continue to give it in the venereal *ardor urinæ.* This practice, however, I am apt to believe, has taken its riſe purely from the name of *ardor* having always been given to the pain in evacuating the urine during the time of a venereal inflammation of the urethra, and the name and virtues of a cooler having always been attributed to this ſalt. But it is certain, that the urine paſſed during the time of a venereal inflammation is no warmer than at other times, and therefore to preſcribe a cooler to allay the heat of it is abſurd;

and I am perſuaded that, on a free and candid examination of this matter, it will be found that nitre has not the ſmalleſt power of alleviating the pain which is then felt; for I have given it in all the different ſtages of this diſeaſe, in ſmall and in large doſes; but from the ſole uſe of it, in a great number of trials, have never been able to obſerve that it afforded the leaſt relief. Nor, when we conſider the cauſe of that pain, and the effects of nitre, have we any reaſon to expect it: for the pain certainly proceeds from the acrid ſalts in the urine ſtimulating the inflamed or excoriated urethra; and a ſolution of nitre applied to any excoriated part, always gives conſiderable pain. For experiment ſake, I rubbed a little of the cuticula from my arm, and, after the ſmarting was over, applied to it ſome cold water. From this I felt no uneaſineſs; but when ten grains of nitre were diſſolved in two ounces of the ſame water, and a little of the ſolution applied to the ſame part, the pain was very conſiderable, and always augmented in proportion as the ſolution was made ſtronger. Experiments aſſure us, that on taking nitre into the ſtomach, the urine becomes impregnated with it. The larger, therefore, the doſes are, the ſtronger will this impregnation be, and the greater ſtimulus added to the urine; ſo that we may reaſonably conclude, that this ſalt will rather augment than diminiſh the pain in evacuating it.

I met with a ſtrong inſtance of this, about a year ago. A young gentleman had got a venereal dyſury, and pretending to cure himſelf, relied ſolely on nitre, which he had taken to the quantity of about ſix drachms per day, in warm cow whey. When I heard how he had treated himſelf, I ſuſpected that the quantities of nitre he took daily had ſuperadded a ſtimulus to that which is naturally in the urine, and occaſioned the increaſe of his pain. I therefore directed him to leave off the nitre altogether, and to make uſe of the ſame quantity of gum arabic in its ſtead; by the uſe of which, diſſolved in large quantities of the whey, he very ſoon got intirely the better of his complaint.

I ſhall finiſh this Eſſay by obſerving, that though nitre may be given in much larger doſes than the preſent practice allows of, yet they ought not to be ventured on without due caution; for there are many weak and delicate ſtomachs which cannot eaſily bear the cold it produces, and others in whom it always creates ſickneſs and nauſea. It will therefore be prudent, when we are not acquainted with the conſtitution, always to begin with ſmall doſes, and rather increaſe them afterwards as we ſhall find occaſion, than raſhly venture on them at once.

EXPERIMENTS

WITH

CAMPHIRE.

AS medical authors have differed ſo very widely in their opinions concerning the nature and effects of camphire (one part of them poſitively affirming that it heats, and another aſſerting with the ſame confidence that it cools the body) I made the following experiments, with a view to have cleared up the diſpute.

EXPERIMENT I.

I took one ſcruple of camphire, incloſed in a little of the pulp of tamarinds. It made no alteration on the height of the mercury in the thermometer at my ſtomach. But twenty minutes after, my pulſe beat only 66; whereas before I took the doſe, it had beat 68: ſome time after this it was reduced to 65. I intended to have meaſured it again, but was obliged to go out, which prevented me.

EXPERIMENT II.

I took two ſcruples of camphire in a little of the ſyrup of pale roſes; which immediately cauſed a ſenſation in my mouth, ſomething like that occaſioned by taking ſtrong pepper-mint water, but much more diſagreeable. On looking at the thermometer at my ſtomach, the mercury, ten minutes after the doſe, was fallen one degree; and my pulſe, which before was at 77, now only beat 75. Twenty-five minutes after the doſe, the mercury was riſen to the ſame height at which it had been before I took it, and my pulſe was again at 77.

Long before this time, however, I began to feel an unuſual laſſitude and depreſſion of ſpirit, accompanied with frequent yawnings and ſtretchings, which ſtole upon me by ſlow and almoſt imperceptible degrees; till, at the end of three quarters of an hour from their firſt appearance, they were grown extremely troubleſome. The mercury in the thermometer remained at the ſame height as it had done before the doſe; but my pulſe was now fallen from 77 to 67.

Soon after this, my head grew ſo very giddy, that it was with great difficulty I could walk acroſs the room; when feeling myſelf, as I thought, ſtifled, I imagined the freſh air would remove that ſymptom, and therefore opened the window and looked out: but every thing in the ſtreet appeared to me in the utmoſt tumult and confuſion; in which imagining that I was involved, I felt myſelf in danger of loſing my balance, and tumbling from my poſition. I therefore ſtaggered from the window to my bed, and having a book with me, read ſeveral pages of it; but had no diſtinct idea of any one ſentence, and far leſs could I connect two or more of them together, ſo as to comprehend the meaning of the author. At laſt, being able to read no longer for the tumultuous motion which I perceived among the letters of the book, and finding it had no power to divert the attention of my mind from the uneaſy ſenſations which diſturbed me, I aroſe to ſee whether I could walk any better; but, to my great mortification, found my head more confuſed, and could hardly walk any at all. I then returned to the bed, and being a little thirſty, called for ſome mutton broth to drink. It being dinner-time, the ſervant, inſtead of bringing the broth, covered the table as uſual, not knowing that I was complaining. When the victuals were brought, I got out of bed again, and with no ſmall reluctance ſwallowed a little of the broth, but could neither taſte bread nor meat, on account of a nauſea, which, however, was not accompanied with any inclination to vomit.

I now ſtaggered again to bed, and took up the book I had left there, in order to make one more effort to divert the attention of my mind from the uneaſy ſenſations I felt; but could not read, as the letters on the book formed only a confuſed group of unſteady images. Self-preſervation now ſuggeſted to me the thoughts of taking a vomit; but as the ſenſations I felt were more of the confuſed kind than of real pain, I was not very apprehenſive of danger, and therefore I reſolved not to evacuate the camphire, but wait patiently to ſee what effect it would have. Hitherto, amidſt a tumult of indigeſted ideas, I had retained ſome ſenſibility; but now the confuſion in my head increaſed ſo much, attended with ſuch a noiſe in my ears, that all knowledge of what was preſent, as well as memory of the paſt, was ſoon intirely loſt in a ſtate of inſenſibility; ſo that I was intirely ignorant of what I did till my ſenſes began to return.

Fortunately, about this time, one of my young gentlemen came into the room, who told me afterwards that I deſired him to ſhut the windows, and then threw myſelf backward on the bed, where I lay a few minutes very quiet—then ſtarted up—ſat on the ſide of it, and made ſome efforts to vomit, but threw nothing up: that I then flung myſelf back again with dreadful ſhrieks—fell into ſtrong convulſions—foamed at the mouth—ſtared wildly—and endeavoured to lay hold of and tear every thing within my reach. This outrageous fit was ſucceeded by a calm ſomething ſimilar to fainting, with this difference only, that my colour was very florid. The ſervants, concluding me to be mad, durſt not come near me, and therefore ſent for my brother, who lived at a little diſtance. When he arrived and ſpoke to me, I awaked, as I thought, from a profound ſleep, and had juſt ſenſibility enough to know him. Soon after came Dr. Cullen, profeſſor of medicine in this univerſity, who had been ſent for alſo. When he had felt my pulſe, which beat 100 in a minute, he ordered me to be blooded; but as natural antipathies will often remain when almoſt every other ſenſation is loſt, that which I have againſt this operation made me obſtinately refuſe to comply; upon which the Doctor went away. All this time, no perſon knew any thing of my having taken the camphire, nor did I recollect it myſelf; and though I was recovered ſo much from the fit I have juſt now deſcribed as to know every one about me, what is ſtrange is, I was intirely ignorant of my own actions, as well as of the place where I was.

At this time, feeling myſelf very warm, I got out of bed, threw myſelf down on the floor, and, thinking myſelf refreſhed by the cold of it, called for ſome cold water, and bathed my hands and face in it. This refreſhed me a little, and in ſome degree quieted a tremor which had ſeized on every part of my body. While I was ſitting on the floor, Dr. Alexander Monro, profeſſor of anatomy, who had alſo been ſent for, came in. I could give him no account of the cauſe of my illneſs; but while he was walking about the room, conſidering what to do, he accidentally caſt his eyes on a paper I had left on the table, containing a relation of my having taken the camphire, and the effects it had upon me, as long as I had remained ſenſible enough to mark them. Upon this he immediately ordered warm water to be got for me, of which having drank plentifully, I ſoon vomited; and though more than three hours had paſſed ſince I had taken the camphire, the greateſt part of it was evacuated, undiſſolved, along with the water.

While I was holding my head over the baſon into which I was vomiting, the ſmell of the camphire aroſe very ſtrong from it; and to this circumſtance it was owing that I firſt recollected I had taken it, though I could give no diſtinct account of the time when, or manner how. The Doctor, after the vomiting, ordered me to drink the juice of two or three lemons and oranges, with a view to correct the too great activity of the camphire that might ſtill remain on my ſtomach; but I was not ſenſible of its having any effect.

I mentioned before that I had not only loſt all remembrance of my paſt actions, but alſo the knowledge of every preſent object; but I now began ſlowly to recover both, though in a manner ſo amazing, that my buſineſs, connections, and every thing of the ſame nature, which I had intirely forgot, at their firſt occurrence ſtartled my mind, as if they were things I had never before been acquainted with: and, what is ſtill more extraordinary, after I knew every one of my family, I did not recollect the uſe of any part of the furniture of my own room; and every object on which I caſt my eyes appeared as ſtrange and new to me, as if I had only that moment begun my exiſtence.

Whether it was owing to the vomiting or to the camphire I know not, but I was now affected with a pretty ſevere head-ach, which diſturbed me a good deal all the evening. Between five and ſix o'clock I aroſe and drank ſome tea, and the juice of ſome more lemons and oranges with water. The giddineſs of my head, ſinging

ing in my ears, exceſſive heat and tremor, which had been ſo ſevere on me before, were now conſiderably abated, though far from being intirely gone off. About ſeven o'clock Dr. Monro returned to viſit me, and found my pulſe reduced from 100 ſtrokes in a minute to 80. We now applied a thermometer to my ſtomach, and in half an hour the mercury aroſe two degrees above blood-warm: it was then changed from my ſtomach to the Doctor's, and in half an hour the mercury fell more than one degree.

Between eight and nine o'clock, feeling myſelf ſtill very much confuſed, I went to bed, and ſoon after fell into a very calm and eaſy ſleep, which continued till next morning with much leſs interruption than uſual. When I awaked, I found my head-ach quite gone, though a little of the confuſion in it ſtill remained. Some time after, upon going to ſtool, I was extremely coſtive, though I had not been ſo before; nor did I feel any thing of it afterwards. All that day I had a very great ſoreneſs and rigidity over my whole body, as if I had been expoſed to cold, or undergone ſome ſevere exerciſe; but this, with all the other ſymptoms, went intirely off in a few days.

EXPERIMENT III.

As the foregoing experiments had not fully enabled me to determine whether camphire acted as a heater or a cooler, I now reſolved to try whether it would give any additional heat or cold to a fluid in which it was diſſolved. Accordingly, having put the thermometer into ſtrong ſpirits of wine, in a few minutes the mercury ſunk four degrees, but would fall no lower, though the thermometer remained almoſt half an hour in the ſpirits. To four ounces of this ſame ſpirits I then added the quantity of camphire directed for making the Spt. Vin. Camphorat. of the Edinburgh Diſpenſatory, and, as ſoon as it was diſſolved, put the thermometer into it again: the mercury very ſoon ſunk to the ſame degree that it had done in the pure ſpirits, but would fall no lower. To this four ounces of camphorated ſpirit I then added half an ounce more of camphire; and that having produced no difference, I added another half ounce; but ſtill the mercury would ſink no lower than it had done in the ſpirit by itſelf.

EXPERIMENT IV.

In pure oil of almonds the mercury ſunk two degrees. After the ſame oil was camphorated according to the Edinburgh Diſpenſatory, it ſunk no lower; and on adding to the ſame oil as much camphire as it would diſſolve, there was no farther alteration produced.

EXPERIMENT V.

The mercury always remained at the ſame height in pure lime-water; as it alſo did after as much camphire was added to it as it would diſſolve. From all theſe experiments it appears plain, that it neither adds to, nor diminiſhes from, the natural heat of any fluid in which it is diſſolved.

When the effect of a medicine does not appear upon fluids with which it is mixed, it becomes no eaſy matter to determine whether it acts as a heater or a cooler; as it may do either the one or the other very conſiderably, without affecting a thermometer applied to any part of the ſurface of the body. A thermometer, and the ſenſations we feel, are the only things we have to judge by. The firſt of theſe, in the experiments here mentioned, could give me no aſſiſtance; and were I to truſt to the laſt, I ſhould certainly conſider camphire as a violent heater; for it augmented very much the velocity of my blood, and made me feel a heat which I had never experienced any thing equal to before: but I would by no means determine poſitively from this, that it acts conſtantly as a heater; for its operation, ſo far as we know of it, ſeems to be very vague and uncertain, as will appear by what follows.

Menghinus gave large doſes of camphire to a variety of animals. It threw ſome of them into a profound ſleep, ſome into a kind of madneſs; on ſome it operated as a cathartic, and on others as a diuretic; to ſome it gave a ſtrange anxiety and ſingultus; and, laſtly, amazingly diſtended the nerves of others, and brought upon them epileptic fits. I could give ſtill more inſtances of its different effects on different, and on the ſame ſpecies of animals; but theſe already mentioned ſeem to prove, that in them it has no conſtant manner of operating. Let us therefore take a ſhort view of its effects upon the human ſubject.

Hoffman mentions a caſe where half a drachm given to a healthy man neither augmented his natural heat, quickened his pulſe, brought on thirſt, or occaſioned any uneaſy ſenſation whatever: and another, where two ſcruples, almoſt as ſoon as ſwallowed, gave a remarkably ſevere head-ach, an extreme coldneſs, pale countenance, languid pulſe, a cold ſweat over the head, loſs of memory, &c.

Monſieur Duteau relates, that one drachm was given to a girl in a very ſevere colic. After taking it, the pain ſoon became eaſier; but it brought on ſuch an extreme cold over all her body, as reſembled death, which could hardly be removed by the aſſiſtance of warm cloaths wrapt round her, and the internal uſe of wine.

To theſe caſes I ſhall only add two more, as publiſhed in a late inaugural diſſertation on the virtues of camphire, by Dr. Griffin. In the firſt, half a drachm was given at eight o'clock in the morning; the principal ſymptoms ariſing from which I ſhall relate in his own words. *Hora decima pulſus, ut ante immoti perſeverabant; ventriculus neque caleſcebat, neque aeſtuabat, ſed hic nauſea, caput vertigine, ita afficiebantur ut ad legendum animum adjicere non poſſet. Jamque mente adeo non conſtabat,*

bat, ut neque pulſus dinumerando, neque quidvis agendo habilis homo eſſet.

Paulo ante horam duodecimam, ita vehemente vomendi conatu agitabatur, ut toto vultu extra propria vaſa iiſſe ſanguis appareret, et tantummodo exiguum aliquid, bile coloratum, et aliquando ſanguine verſicolore interſperſum, vomitu rejiciebatur, totum robur, maxime artuum inferiorum amittebatur, et ipſe vacillans titubare incipiebat, inter vomendum pulſus parvi, languidi, multoque naturalibus citatiores, octogeni in ſingula minuta comperiebantur, &c.

In the other caſe the doſe was two ſcruples, taken alſo at eight o'clock in the morning. It is as follows: *Horæ dimidio vix preterito, moleſtum in ventriculo ardorem perſentiebat, hora nona, pulſus quaternis vel quinquinis per ſingula minuta rariores erant, quam eſſe conſueverant. Hora decima ventriculi ardor et nauſea propterea, ſicut auguror, quod jentaculum acceſſerat minus moleſti ſentiebatur: pulſus ſenis vel ſeptenis numero decreſcebant. Hora undecima, homo*

homo oſcitare et ſomno peti incipiebat, quem tamen ſuſceptum, ventriculi aeſtus et capitis vertigo interpellebant. Vertigo per intervalla nunc ingraveſcebat, nunc iterum prorſus evaneſcebat. Ille modo ſomno obrutus jacebat, modo quaſi ab inſomnio experrectus exiliebat; interdum quaſi ebrius titubabat, et corpus libratum male tenebat: adeoque omnia cogitata, omnes animi imagines turbabantur, ut ſaepius conatus, pulſum numerum vix referre poſſet. Hi autem denis vel duodenis in ſingula minuta infra naturæ modum, toto corpore levius frigeſcere ſentito, et vultu palleſcente peragebantur.

From all theſe caſes it does not appear that any inference can be drawn ſtrong enough to demonſtrate that camphire acts as a cooler. That which ſeems to favour it moſt is the caſe of the girl mentioned by Duteau: but even that, when we conſider it ſeriouſly, will not appear in the ſame light as it does when we only take a ſlight view of it; for it is evident that the effects of the camphire were ſo ſtrong as to throw the girl into a violent fainting fit; and every one accuſtomed to ſee faintings muſt be abundantly ſenſible, that a want of circulation, cold ſweats, and chillineſs over all the body, are the ſymptoms that generally accompany them; and theſe ſymptoms are more or leſs ſtrong, according to the ſeverity of the fit. It appears, therefore, that the immediate cauſe of this coolneſs depended on the fainting having obſtructed the circulation, and not upon any frigorific power of the camphire itſelf. To illuſtrate this ſtill farther, I ſhall obſerve, that I have ſeen ſeveral inſtances, where the immoderate uſe of vinous or ſpirituous liquors has thrown people into cold fainting fits, has diminiſhed the number of pulſations, and almoſt totally obſtructed the circulation. Theſe, I think, are parallel caſes; but ſurely no perſon would infer from them that, becauſe fainting and cold ſweats ſometimes ſucceed the immoderate uſe of vinous and ſpirituous liquors, they act as coolers, when every day's experience demonſtrates the contrary,

trary, and teaches us that they diffuſe a genial warmth and vigor through our bodies.

From the reſt of the caſes I have mentioned, nothing of conſequence can be drawn, relative to the heating or cooling virtue of this drug; but from all of them it is evident, that it has a very ſtrong tendency to affect the nerves, as large doſes of it always produced convulſive ſpaſms, giddineſs of the head, and almoſt every other ſymptom of the nervous kind. It appears alſo that it has a pretty ſtrong ſomniferous power; and, as far as I know, whatever poſſeſſes that power, has a power of heating alſo. Opium, the ſtrongeſt ſoporific we are acquainted with, very conſiderably heats the conſtitution, and, if taken in large doſes, produces convulſions ſimilar to thoſe produced by camphire *. Strong liquors, too, often pro-

cure

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* I have juſt now in my poſſeſſion a caſe by Dr. Clerk, phyſician in this city, where ʒij of crude

opium

cure ſleep; but they alſo heat, ſtimulate, and bring on convulſions. Camphire procures ſleep, and brings on convulſions: may we not therefore conclude that it heats alſo? Coſtiveneſs alſo ſeems to be a pretty conſtant effect of camphire, and affords another corroborating proof of its being a heater; as all the medicines which poſſeſs this power bring on thirſt, create a dryneſs in the throat and fauces, and accelerate the motion of the blood. Farther, if I may be allowed to add reaſoning from analogy to the ſenſations which I felt after taking it, I cannot help being of opinion that it acts as a heater; and I am perſuaded that analogy and ſenſation, though they do not amount to a plain certainty, yet prove almoſt enough to

opium brought on a train of violent convulſions, very much reſembling thoſe which I and ſome of the gentlemen in the caſes I have mentioned, were attacked with; which to me affords a kind of proof, that camphire, as well as opium, is a heater, as their method of affecting the body is very much alike.

convince a mind not entirely led by prejudice, or devoted to ſcepticiſm.

From the above experiments, no certain rule can be laid down to determine the exact quantity of camphire which may be given at a doſe. It would appear, however, at a medium, to be between twenty and thirty grains: for Hoffman mentions a caſe where twenty had no effect; and the ſame quantity in my firſt experiment did not operate in any ſenſible manner. But there are ſeveral inſtances on record, where thirty grains have operated by much too violently: therefore, though twenty may poſſibly be taken with impunity, I think it will always be prudent to begin with a leſs quantity, as we can eaſily increaſe it if we find occaſion, but may perhaps find it beyond our power to remedy the bad effects which may be occaſioned by giving too much.

I ſhall finiſh what I have to ſay on this ſubject by obſerving, that experiments of this or a like nature are the only ſure methods to lead us into a diſcovery of the real virtues and effects of medicines; to eſtabliſh certain determined ideas of their operation; and to enable us to preſcribe them with more reaſonable hopes of ſucceſs than we have hitherto done.

ESSAY III.

On DIURETICS and SUDORIFICS.

DIURETICS are a very uſeful and neceſſary claſs of medicines, and, when properly managed, may often anſwer the moſt valuable purpoſes. It would therefore be no inconſiderable improvement to the healing art to determine, as near as poſſible, their relative powers. I am well aſſured, that to do this with any tolerable degree of preciſion is very difficult; and to determine it with a mathematical exactneſs altogether impoſſible. We may, however, attempt to do it in the beſt manner that our own powers, and the circumſtances of the caſe, will admit of. With this view I made the following experiments.

I firſt weighed the whole quantity of urine that I made from nine o'clock in the morning to two o'clock in the afternoon, after I had drank one pound ſeven drachms and a half of ſimple infuſion of Bohea tea (the exact quantity that it took to fill a bowl out of which I uſually breakfaſted). This I did ſeveral times; and (for reaſons that will be afterwards mentioned) though the ſame quantity of tea was always drank in the morning, I found the quantity of urine made ſome forenoons very different from what was made in others. I therefore thought it would come neareſt the truth to take one-third of the whole quantity made from nine o'clock to two, in three different forenoons, as the ſtandard of judging by in my future experiments. Each of the diuretics mentioned below were likewiſe taken three different mornings in the above-mentioned quantity of tea. And the quantities of urine marked in the following table, as well when the tea was taken alone, as when the diuretics were taken along with it, is always to be underſtood as one third of the whole that was evacuated at three different trials.

A Table of the different Quantities of Urine always diſcharged in an Equal Time, viz. from Nine o'Clock in the Morning till Two o'Clock in the Afternoon, when an Equal Quantity of the ſame Liquid was drank, but with different Diuretics, in different Quantities, diſſolved in it.

	℥	ʒ	℈
By ℔i ʒ vii ß ſimple infuſion of Bohea tea, ſtandard - - -	15	4	
By do. with ʒij of ſalt of tartar - -	22	7	2
By do. with ʒij of ſal nitre - -	22		
By do. with 4 drops of oil of juniper	20	3	
By do. with ʒi of ſalt of wormwood	19	7	1½
By do. with ʒij of Caſtile ſoap	19	1	1
By do. with a tea-ſpoonful of ſpt. nitr. dulc. - - -	17	6	1½
By do. with 15 drops of tinct. cantharid. - -	16	4	
By do. with ʒij of ſal polychreſt.	16	3	
By do. with ʒ ß of uva urſi -	16	1	½
By do. with ʒi of magneſia alba	15	5	
By do. with ʒij of cream tart. -	10	2	½

A Table of the different Quantities of Urine evacuated in the ſame Space of Time, after drinking the ſame Quantity of different Liquors.

	℥	ʒ	℈
By ℔ i ℥ vii ß of weak punch with acid -	21	2	0
By ditto of new cow whey - - -	18	6	0
By ditto of decoct. diuret. pharm. Edin.	17	5	0
By ditto of London porter - - - -	16	7	0
By ditto of decoct. bardan. pharm. Edin.	14	7	0
By ditto of warm water-gruel - -	14	6	2
By ditto of ſmall beer - - - -	13	7	1
By ditto of warm new milk - - -	11	7	0

Though ſome of the medicines here made trial of, ſeem to poſſeſs a much greater power of evacuating by urine, than others; yet there is no poſſibility of determining with any degree of certainty, the exact quantity of it that any given quantity of a diuretic will cauſe to be evacuated from a given quantity of any liquor; nor the exact ſuperiority that one diuretic has over another. For, during all theſe experiments, I conſtantly found, that in proportion to the heat of the weather, the quantity of urine decreaſed, and *vice verſa*. I found it alſo conſtantly decreaſe, in proportion to the quantity or ſeverity of the exerciſe I uſed, and *vice verſa*; ſo that it appears to be one of the invariable laws of the animal œconomy, that when a given quantity of any liquid is drank, the quantity of urine ſecerned from the blood in a given time, will always be greater or leſs, according to the heat of the weather, the exerciſe that is uſed, or reſt that is taken, always having a regard to the diuretic power of the liquid; for a ſtronger diuretic will evacuate more in a heat a little above what is natural to the conſtitution, than a weaker one will do in a heat a few degrees below what is natural to it. The reaſon of warmth and exerciſe operating in this manner is obvious, for they increaſe the cuticular diſcharge; and the greater the quantity of the animal fluids that paſſes off through the pores is, the leſs will conſequently remain to paſs off by urine. Hence it appears, that unleſs a perſon during a courſe of experiments on diuretics, could remain in an equal degree of heat, without any exerciſe; and unleſs the evacuations were to remain exactly in their natural ſtate, and preſerve a conſtant and uniform proportion to each other, it will be impoſſible to tell the exact quantity of urine evacuated by any medicine whatever, or the exact relation of diuretics to one another.

Boerhaave, and a few more writers on the *materia medica*, have mentioned, that the fixed neutral ſalts, and other diuretics, may be managed ſo as to prove ſudorifics. Few, however, have attended to this hint; and pharmaceutical writers ſtill continue to divide them into two diſtinct claſſes; which is certainly ſuperfluous, as they both operate exactly in the ſame manner. For the *ſp. mindereri*, one of the moſt powerful ſudorifics, evacuates very plentifully by urine, and does not in the leaſt provoke ſweat, if inſtead of giving it with warm liquids, and covering the body in the uſual manner, it be given with cold liquids, and the perſon who takes it kept in a cool place; and, on the other hand, the ſalt of tartar and nitre, though among the moſt powerful diuretics, when taken with large quantities of warm liquids,

quids, if the body be well covered, prove excellent ſudorifics, and do not increaſe the quantity of urine; ſo that from theſe facts, which are the reſult of repeated experiments, I think it ſeems plain, that the nature of diuretics and ſudorifics is exactly the ſame; and that their conſtant manner of operating, is always to increaſe the fluid ſecretions, without having any power or propenſity of directing them to this or that emunctory: which power ſeems to depend intirely on the warm or cold liquors that are uſed, and the regimen that is obſerved during their operation.

The ancients had an opinion (and it is not long ſince it was intirely exploded) that a variety of particular medicines had only a particular power of operating on ſuch and ſuch humours. On this theory they had a particular remedy for every humour, and for every part of the body: thus one thing evacuated bile, another phlegm, and a third water. We now ſee the abſurdity of this reaſoning, and are well aſſured that medicines do not act by ſuch partial, but by general laws; and that an evacuant will throw off indiſcriminately every thing that comes in its way: that an attenuant will thin and divide the particles of every thing with which it comes into contact and is miſcible. We know that the power of nature is ſuch, that whatever is properly fitted for excretion, is thrown out of the body, as ſoon as poſſible, by the proper emunctories, fabricated by her for that purpoſe. Now, as the urine and ſweat are pretty ſimilar as to their fluidity, and as the organs through which they paſs are pretty ſimilar alſo; and as nature generally takes the ſhorteſt and ſimpleſt method to perform all her operations, the fluid ſecretions will be thrown off by the ſhorteſt paſſage, which is by the bladder; but if warmth be promoted by cloaths, by drink, or tepid vapour, and thereby the cuticular pores relaxed, then an eaſier paſſage is found by them, and ſo what is fit to be ſecreted goes off in this way; ſo that a medicine which would have operated as a diuretic, for this reaſon operates as a ſudorific, and *vice verſa.*

Though I have ſaid before, that diuretics and ſudorifics are exactly of the ſame nature, and operate in the ſame manner, I would only be underſtood to mean that part of the claſs of diuretics which has an attenuating aperient quality; for we have many things included in it by pharmaceutical writers, which have no ſuch quality; and though they ſometimes procure a plentiful diſcharge of urine, have not the ſmalleſt title to the name of diuretic. Thus, if the urethra or ſphincter veſicæ is contracted by a ſpaſm, warm emollient fomentations applied externally will often relax it, and allow the urine to come away. But does the fomentation on this account deſerve the name of a diuretic, any more than the ſurgeon's catheter, which puſhes back a ſtone or gravel obſtructing the paſſage, and thereby opens a vent for the urine alſo? Certainly it does not; and therefore attenuating and aperient medicines are the only ones that, literally ſpeaking, can be called diuretics. Of this kind were thoſe that I made uſe of in the experiments contained in the firſt table of their relative virtues; though I muſt beg leave to take notice, that they, and all others of a ſimilar nature, can only be made uſe of when the ſecretions are not duly carried on, or the fluids are too viſcid and tenacious to be ſtrained through their proper channels, and would be highly improper, and hurtful, if given when the paſſages are obſtructed by gravel, or ſtraitened by ſpaſm; as they would, in the firſt caſe, increaſe the quantity and momentum of the urine, againſt a place too impenetrably ſhut up to be opened by any force of that kind; and in the ſecond, by increaſing the ſtream of urine, increaſe the irritation; whereby the ſpaſm would become ſtill the more obſtinate.

Thoſe things whoſe relative virtues are contained in the ſecond table, are moſtly ſuch as are made uſe of for the ordinary purpoſes of life; though ſome of them appear to have conſiderable diuretic virtues, to compare which with one another, was my principal deſign in taking them.

EXPERIMENTS

ON

SUDORIFICS.

SINCE the time of the illuſtrious Sanctorius, who by his ſtatical experiments made it evident to a demonſtration, that the quantity of matter which paſſes off by inſenſible perſpiration is very large; obſtructions of the cuticular pores have always been reckoned one of the principal cauſes of the moſt acute, as well as chronic diſtempers: on which account, a variety of medicines have been from time to time introduced into practice, in order to remove them when they happen.

From the earlieſt time that medicine began to be a regular ſtudy, it was obſerved, that the criſis of almoſt every acute diſtemper happened by a ſweat; which naturally led mankind to imagine, that if they could by artificial methods procure a ſweat, they would bring on the wiſhed-for criſis. They had alſo obſerved, that perſons expoſed to great heat were generally found ſweating; and this led them to endeavour to procure it in the ſick, by means of prodigious loads of bed-cloaths, hot, volatile, alkalious, ſpirituous, and, as they are termed, alexipharmac medicines; by a liberal uſe of which, many patients have been deſtroyed, while no ſweat could be procured; or melted down by too profuſe an one.

This roaſting practice, though once firmly eſtabliſhed, and long held ſacred and indiſputably right by all parties, is now loudly complained of by the ſenſible part of phyſicians and others; and has, perhaps, been loſing ground ever ſince the days of the illuſtrious Sydenham, who was the firſt that dared openly to attack it, with no ſmall riſque to his reputation, from a ſet of deſigning intereſted men, who would rather ſee mankind perſiſt eternally in error, and die of miſconduct, than be ſaved by the truth, if it detracted a mite from their annual profits. In Britain I am ſure it is faſt declining, and hope the new method of inoculation will open the eyes of mankind, and demonſtrate its impropriety. But though it be declining, there is ſtill enough of it left to do a deal of miſchief. Much has been of late wrote, many ſenſible arguments, and ſtill more convincing inſtances have been brought againſt it; all which, it is hoped, have had conſiderable weight. But to ſet its diſadvantages ſtill in a clearer light, by proofs drawn from experiments, has not hitherto been attempted, and is therefore the ſubject of the following trials.

Dr. Huxham gives it as his opinion, that great heat, and too rapid motion of the blood, hinder it from giving of the natural ſecretions; and ſeveral inſtances have occurred to me, which, as far as I can judge, prove it to a demonſtration. I was therefore led to conclude, that the only way to produce a ſweat, was to leſſen the heat by means of cold liquids; and my firſt eſſay of this nature was upon myſelf.

EXPERIMENT I.

My conſtitution has always borne a large quantity of ſtrong liquor very badly: its conſtant effect has been to throw me into a kind of temporary fever, which ſoon after I went to bed began, and in a little while after augmented to ſuch a degree, that my ſkin became hot, rigid, and dry, my tongue parched, and an intenſe heat over all my body. In this condition I uſed to paſs the night, reſtleſs and toſſing; and nothing ever gave me relief till a moiſture appeared on my ſkin, to procure which I had long relied ſolely on the uſe of warm diluting drinks, tho' they generally diſappointed me. At laſt, conſidering that their ill ſucceſs was probably owing to their augmenting the increaſed heat of my body, I reſolved to try a different method; and ſoon after, having drank a pretty large quantity of liquor on purpoſe, I went to bed, and had a large bowl of cold water ſet by me. As ſoon as I felt the uſual heat and reſtleſsneſs, I took a very large draught of the water: about ſix or eight minutes after, I was agreeably ſurpriſed to find a ſweat breaking out upon me. I kept myſelf in one poſture to encourage it; ſo that in a little time it became very profuſe, and intirely relieved me.

EXPERIMENT II.

As I was not quite certain whether this laſt ſweat had been occaſioned by the cold water, or owing to accident; in order to clear up the doubt, the next night, after ſupper, I drank a bottle of port wine, and went to bed. The heat and fever ſoon after came on as uſual. I had taken a ſmall pocket thermometer along with me, which I applied to the pit of my ſtomach, and in twenty minutes found the mercury riſen to 110 degrees, and my pulſe beating 94 or 95 in a minute. I had a bowl of cold water ready, and in this ſituation took a large draught of it, and ſoon after another. In eight minutes after the firſt draught, my ſkin, which before was dry and parched, began to be moiſt; and in eight or ten minutes more I was in a profuſe ſweat. On looking at the thermometer, I found the mercury fallen 2 degrees; and half an hour after, though I ſtill continued ſweating, it was fallen 3 degrees more. My pulſe, which, before the ſweat came out, had beat about 94, now beat only 85.

Theſe experiments ſeem clearly to prove, that there is a certain degree of heat (which may be called the ſweating point) always abſolutely neceſſary to produce that evacuation; and that the farther the heat of any perſon is advanced above, or reduced below, this ſtandard, the farther he is removed from any poſſibility of ſweating. But although there is a ſtandard degree of heat, at which, and perhaps at no other, a ſweat can be produced, yet we may reaſonably conclude that this degree is not the ſame in all perſons, nor in the ſame perſon at all times; but that it rather differs according to the difference of conſtitutional heat, and other circumſtances.

If there is therefore an exact ſweating point in every perſon, this eaſily explains to us the reaſon why cold water often acts as a ſudorific: for if the heat of the perſon who takes it be at that time conſiderably above the ſweating ſtandard, a ſufficient quantity of the water will reduce it to the ſtandard, and ſo procure the ſweat: and warm water, or any warm liquid, will have the ſame effect when the heat is below it. It is upon this principle, and no other, that we can give a reaſon why a large draught of cold water, earneſtly longed for by the patient, has often been the happy means of an almoſt inſtantaneous ſweat in ardent inflammatory fevers, after all the common warm methods had been attempted in vain. It would therefore ſeem, that the practice of denying the uſe of cold liquids to people in theſe diſtempers, is ſo far from having its foundation in reaſon or the nature of things, that, after proper examination, it will be found pernicious and ridiculous.

Whenever a perſon has a ſtrong, full, and frequent pulſe, attended with great thirſt, a parched dry tongue, and a violent ſenſation of heat, cooling medicines ſeem plainly to be indicated by nature; and, purſuant to her indications, phyſicians have time immemorial been accuſtomed in theſe caſes to preſcribe them. But, which is amazing, even when the ſtrongeſt coolers have been indicated, and even when they have taken the greateſt pains to ſelect them, they have always given them in a warm vehicle; ſo inconſiſtent is the practice of phyſic often with itſelf, and in this caſe, I think I may add, ſo irreconcileable to reaſon and ſenſe. The patient himſelf may often feel a very great heat and thirſt, his tongue may be parched and dry, and yet the heat may be below the ſtandard of health;

therefore the proper exhibition of coolers requires caution and judgment, as in this caſe they would certainly do hurt. But when along with theſe ſymptoms there is a ſtrong, frequent pulſe; when the mercury in a thermometer applied to the ſurface of the body, ariſes very conſiderably above the degree of blood-warm; I would then venture not only on the uſe of cold water alone, but alſo on giving the ſtrongeſt coolers along with it: I think I ſhould only follow what nature pointed out to me, in ſo doing.

EXPERIMENT III.

Some time after theſe laſt experiments, being affected with a ſlight rheumatiſm, one night, after I was in bed, having reſolved to try a ſweat, in order to procure it, I drank ſome large draughts of warm cow whey. In about twenty minutes a moiſture on my ſkin began to appear, at which time I was very warm. On looking at the thermometer, I found the mercury was ariſen to 108 degrees; and my pulſe beat 86 in a minute. The ſweat ſoon became very profuſe. After it had laſted about half an hour, I found the mercury fallen one degree and a half, and my pulſe reduced from 86 to 81. After I had continued an hour in the ſweat, the mercury had fallen one half degree more, and my pulſe beat only 74. I now increaſed the ſweat by more draughts of the tepid whey, and found my pulſe ſtill diminiſh, till it came to about 70, near which it remained for about an hour; when, being conſiderably exhauſted with the evacuation, I became a little faint. My pulſe was now quick and weak; and not long after (though the fainting was almoſt gone off) it grew quicker, weaker, and fluttered; and the mercury had fallen almoſt another degree.

The ſame experiment was tried on another ſubject. His heat and pulſe were alſo higheſt before the ſweat broke out. Afterwards the appearances were pretty much the ſame as thoſe which happened to me; only the ſweat was not puſhed ſo far, and therefore was not attended with the quick weak pulſe.

What I have juſt now related, may perhaps lead us to diſcover the reaſon of thoſe cold ſweats which often immediately precede death, and are, in acute diſtempers, generally fatal, whenever they make their appearance: for, from what happened to myſelf and to the other gentleman, it is plain, that a briſk circulation and great heat are by no means neceſſary to keep up a ſweat once begun. When a perſon therefore is weakened by a diſeaſe, when the pores are open by previous ſweating, and when, towards the cloſe of life (this weakneſs increaſing, and the little remaining ſtrength being nearly exhauſted) the ſkin loſes all its elaſticity, its ducts allow the ſerous parts of the blood to paſs through them, without almoſt any propelling force; which ſerous parts now partake of the cold of the reſt of the maſs, and appearing on the ſurface of the ſkin, form what is called a cold ſweat.

In theſe experiments it appeared by the thermometer, that the natural heat ſinks in proportion to the ſeverity and continuance of a ſweat; and I have ſince found by ſeveral others, that a perſon whoſe natural heat, at the beginning of a ſweat, ſhall be able to raiſe the mercury to 108, or even 110, after the ſweat has continued for ſix or ſeven hours, ſhall not be able to raiſe it to the natural degree of blood-warm. This certainly ſhould teach us to be very cautious in urging this evacuation too far, eſpecially when we are any way doubtful of the natural ſtrength; for ſhould we put a patient into a profuſe ſweat, where it is too little, we ſhould commit a moſt egregious, and perhaps irrecoverable, blunder: or ſhould we exhauſt nature by the ſame means, for an ailment which we only take to be trifling, and ſhould this ailment prove to be an acute diſtemper, we ſhould act the ſame fooliſh and inconſiſtent part, as a general who ſhould draw out the greateſt part of the ſoldiers from a garria garriſon juſt going to be beſieged by a potent enemy. We are cautioned by the beſt practical authors, in the ſevereſt manner (and certainly the caution can never be too ſtrongly urged), to be ſparing of the vital fluid, and never to uſe the lancet in low nervous diſtempers, but when obliged to it by abſolute neceſſity. Many of them have alſo cautioned us againſt profuſe ſweating, but in a manner which ſhews they are much leſs afraid of it than of the other; though I am fully perſuaded that the danger is altogether as great: for whoever conſiders the languor and immenſe proſtration of ſtrength that he has felt after long continued and profuſe ſweating (perhaps only to remove ſome pain accompanied with little or no ſickneſs) will, I dare ſay, agree with me, that two or three of theſe ſweats have weakened him more than the loſs of twelve, fourteen, or even twenty ounces of blood. As a proof of this, I ſhall mention what happened to myſelf. My rheumatiſm being not at all relieved by the ſweat mentioned in the laſt experiment, ſome days after I was blooded pretty largely; which though it gave but little relief, no ſenſible proſtration of ſtrength enſued, no loſs of colour, nor any languor or inclination to faint: but ſome time after, on the pains growing worſe, I lay and ſweat in bed for the greateſt part of three days. After this my ſtrength was quite exhauſted, my ſpirits ſunk, my eyes hollow, my colour loſt, and I was only able to ſtagger with difficulty through the room. I ſhould not lay ſo much weight upon this, as it happened to myſelf, if a variety of ſimilar inſtances had not occurred in other people alſo; all which taken together, ſeem plainly to prove, that even when there is little or no ſickneſs, two or three violent ſweats will weaken a perſon much more than the loſs of a pretty large quantity of blood. And if they do ſo in caſes attended with almoſt no ſickneſs, what muſt be the conſequence when they are urged too far, in perſons already waſted with acute or chronic diſtempers?

That profuſe ſweating is more deſtructive to the natural heat and ſtrength than even pretty large blooding, is a truth which ſeems never to have been ſufficiently attended to in practice; and it is no very uncommon thing to ſee a perſon thrown into a large and continued ſweat, without any apprehenſion of danger, when at the ſame time were he to loſe one ſingle ounce of blood, it would be reckoned highly imprudent, as detracting from that ſtrength which ought to have ſupported him in the diſeaſe. How far this is reconcileable to common obſervation, and the feelings of every one who has been in theſe circumſtances, I ſhall leave the judicious to determine.

Dr. Huxham, that careful obſerver of nature, is the only author I have met with who ſeems to have been fully aware of the fatal conſequences of large ſweating in low putrid diſtempers; and accordingly exclaims againſt it in the keeneſt and moſt nervous manner, as having a very direct tendency toward the deſtruction of the patient. But I carry the matter ſtill farther, and affirm, that in all diſtempers whatever, profuſe ſweating too long continued, may have the ſame effect; and that it ſeldom or never can be uſeful, as all the purpoſes of it may be fully anſwered by a gentle mador on the ſkin, which may be much longer continued with leſs hurt to the ſtrength of the patient.

From what I have juſt now ſaid, I would not have any perſon infer, that I condemn the uſe of ſweating altogether. I would only be underſtood to mean, that it is too often indiſcriminately ordered, without duly weighing the conſequences that may ariſe from it, and never ought to be ventured on but with caution and judgment; for if the evacuation by perſpiration in its ordinary ſtate is ſo great as Sanctorius and Dr. Keil make it by their experiments; what muſt it be when urged the length of a profuſe ſweat? and if the evils ariſing from its being a ſhort time obſtructed, are ſo very great, may we not expect great ones alſo to follow, from its being urged as it were with violence through every pore? A ſtrong athletic perſon, indeed, in tolerable health, can hardly ever ſuffer much from a moderate, or even from a profuſe ſweat; but let it be often repeated in bed, or long continued at a time, and the moſt robuſt conſtitution will ſoon ſink under it.

It may be ſaid here, that the bottle-maker, the cook, the day-labourer, &c. are ſo many ſtrong objections againſt what I have above advanced. But let it be conſidered that they work in the open air; and experience ſhews us, that the ſame perſon can bear twice as much ſweating in the open air, as he can do ſhut up in a room, or in a bed. Beſides, theſe people generally eat and drink heartily; whereas, when ſweating is ordered as a medicine, the patient is generally condemned to water-gruel and ſlops, and lies in bed immerſed in his own ſweat, as in a warm bath, whereby the fibres are relaxed,

laxed, and by degrees loſe their natural firmneſs and tenſion.

As we ſee from the above experiment, that toward the end of a large and long continued ſweat, a quick, weak, tremulous pulſe comes on; whenever we meet with one of this kind, we ought to conſider it as a ſtrong indication of the weakneſs of nature, and therefore, in my opinion, to be nearly as cautious of ſweating as of blooding. This alſo ſhews the abſolute neceſſity of ſupporting a patient under copious ſweats, by the ſtrongeſt and moſt exhilarating cordials (except in caſes where we want to reduce the natural ſtrength). For this purpoſe, I would recommend ſtrong broths, and beef tea, perfectly cleared from fat; but above all, a liberal uſe of generous red wines, which, properly managed, will greatly exceed every cordial medicine with which the ſhops are furniſhed, keep up the ſpirits much better, and at the ſame time be leſs liable to heat the patient. In this opinion, I know that I am contradicted by the general cuſtom, which is ſo abſurd, that I have many times ſeen a patient ſtrictly forbidden the uſe of a little wine, and at the ſame time moſt liberally crammed with volatile alkalious ſalts and ſpirits, alexipharmac boluſes and mixtures, &c. This inconſiſtency is by much too groſs to have exiſted upon any other baſis than that of cuſtom or ignorance, and will never ſtand the teſt of ſound judgment and impartial enquiry: it is now loſing ground apace, and I muſt ſay, for the honour of my country, that I have ſeen it much leſs practiſed here than in England; where the apothecary, ſeldom or never paid for attendance, finds it his intereſt to pour as many medicines into the ſtomach of his patient, as he can poſſibly get him to ſwallow.

EXPERIMENT IV.

In the middle of winter, after having been long abroad in a very cold day, I was ſeized in the evening with a ſevere fit of trembling and ſickneſs. On being attacked I went to bed, and with a view to ſhorten the cold fit, drank ſeveral large draughts of warm water gruel; it continued, however, about an hour; after which it gradually gave way to a ſucceeding warmth, which ſoon augmented to an intenſe heat, accompanied with a dry, parched ſkin, and great thirſt. In this ſtate I applied a thermometer to my ſtomach, and in twenty minutes the mercury aroſe to 112 degrees, which is two degrees above the heat of a common fever. In order to promote a ſweat, which I knew would relieve me from the intenſe heat and reſtleſſneſs, I continued frequently taking large draughts of the gruel for about an hour after the heat began; but it ſtill continued, no ſweat was likely to appear, and my pulſe, as far as I could judge, was above 100. Being in the utmoſt anxiety, and very reſtleſs, I reſolved to try if leſſening the degree of heat would bring on the wiſhed-for *diaphoreſis*; and with this intention drunk two or three large draughts of cold water, which run off very plentifully by urine, but produced no ſweat; from which I concluded that the pores of my ſkin were too ſtrongly obſtructed to be opened by any effort *ab intra*, and therefore immediately ordered a large piece of flannel to be wrung out of boiling water, and wrapped round my legs and thighs. In leſs than five minutes after this application the ſweat began to appear over all my body, and ſoon after grew very copious. When it had continued about twenty minutes, my pulſe was fallen to about 96 or 97. After an hour and a half's continuance my pulſe was at 85, and the mercury was ſunk three degrees.

EXPERIMENT V.

Having ſucceeded ſo well with the warm flannel, I reſolved ſome time after, when I was in perfect health, to try whether it would procure a ſweat without the aſſiſtance of any diluting liquor; and accordingly had a large piece of it wrung from boiling water, and wrapped round my legs and thighs, as in the laſt experiment. In about ſeven or eight minutes a ſweat began to appear, and ſoon ſpread itſelf over all my body. My pulſe, which before the application of the flannel was at 72, only aroſe to 77; and the mercury in the thermometer at my ſtomach, which before ſtood exactly at the degree of blood-warm, only roſe 2 degrees higher. After the ſweat had continued about half an hour, my pulſe was reduced to 74, and ſoon after to 70: at this time alſo the mercury was fallen one of the degrees it had riſen before. The ſweat was now decreaſing very faſt, and almoſt intirely gone; on which account I took a large draught of warm water-gruel, by the aſſiſtance of which, and heat, it ſoon returned, and continued till next morning.

On comparing experiment IV. with the former ones, where cold water had operated ſo powerfully, it appears that it had not here the ſame effect. But the reaſon is obvious: my pores had been ſo imperviouſly ſhut up by the cold, that the water-gruel had not been able to force its way through them, and had conſequently taken to the kidneys, where it found an eaſier paſſage, and produced a *diureſis*; which however was but trifling till I drank the cold water, and then became immediately moſt profuſe; ſo that the whole force of theſe liquids was now directed this way, and little or no effort made on the ſkin. This affords a valuable hint with regard to ſweating, viz. never to perſiſt too long in our endeavours to urge it by the internal uſe of liquids, after we find that they have very conſiderably augmented the quantity of urine; for if we do, our endeavours will probably be in vain. In this caſe, if we find a ſweat abſolutely neceſſary, perhaps the only method of procuring it will be by a warm vapour or bath. The ſame obſervation takes place in violent *diarrhœas*, where it often happens that every thing given with an intention to raiſe a ſweat, inſtead of doing ſo, only increaſes the inteſtinal diſcharge: and the ſame method of cure muſt be obſerved, with this difference only, that opium, if it can be given with impunity, will not only leſſen the *diarrhœa*, but alſo, in conſequence thereof, turn the perſpirable matter toward the ſkin, and ſo produce a diaphoreſis; whereas, when the liquids given to raiſe a ſweat once get a vent by the kidneys, I have never found that opium had the ſmalleſt power of reſtraining them.

From both theſe experiments we may learn, that this method of ſweating by warm wet flannel is much more eaſy and expeditious than the common one of doing it by large quantities of warm liquors, and exceſſive loads of bed-cloaths; and, if cautiouſly managed, can be done without the leaſt danger of cold: for I have found, from repeated trials, that there is no neceſſity of wrapping the flannel round the whole body, as is commonly done, but only round the legs and thighs, from whence it can be more eaſily taken away, as ſoon as the ſweat is come out; for if it be allowed to remain long after, it will be apt to grow cold, and check that evacuation it had before promoted.

From experiment V. it ſeems evident, that the natural heat is much leſs augmented above its uſual ſtandard by this way of ſweating than by the other. It would therefore ſeem preferable, in all caſes where we want to raiſe a ſweat with as little augmentation as poſſible to this heat, and to the momentum of the blood. But it appears alſo from this experiment, that though we can eaſily raiſe a ſweat by the help of warm wet flannel, yet we cannot continue it by the ſame means; for, unleſs the patient be ſupplied with ſome liquid, all the fluids in the body that are fit for excretion muſt ſoon be evacuated, and ſo an end be put to the ſweat; which we muſt endeavour to prevent, by giving from time to time draughts of any tepid liquor, which ought always to be made warmer, in proportion to the continuance of the ſweat, and conſequent decreaſe of the natural heat; for I have found it an eſtabliſhed fact, that though cold water will almoſt always bring on a ſweat, when the conſtitution is much above its natural degree of heat, yet when this ſweat has continued long enough to reduce the heat below, or even near to its natural ſtandard, a draught of the ſame cold water, which brought on the ſweat at firſt, ſhall now put a final ſtop to it.

It will be proper to obſerve here, that though the method of ſweating by warm wet flannel has its particular advantages, it has alſo ſome diſadvantages attending it; for when the whole body is wrapt in it, or even the legs and thighs only, the perſon, by the vapour, and by the profuſe ſweat which it generally brings on, is in a ſituation nearly ſimilar to that of a warm bath, whereby the fibres become very much relaxed, and ſoon loſe their natural firmneſs and tenſion, the muſcles become flabby, and a languor and debility follows, in proportion to the time that the patient is kept in this manner, and to the quantity of the evacuation. It ſeems therefore plain, that though this method may be uſed with ſucceſs in rheumatiſms, colds, or even in fevers, when the natural ſtrength is but little impaired, it ſhould never take place where the pulſe is weak, where the ſymptoms of debility begin even to appear; and far leſs towards the laſt moments of life, when nature ſinks down apace to her final diſſolution, and has much more need of ſomething to ſupport than to exhauſt her. I ſhould not have thought it neceſſary to have given a caution againſt a practice ſo very inconſiſtent with reaſon, had I not ſeveral times ſeen it ordered, and as often accelerate death.

EXPERIMENT VI.

I prevailed on an athletic labouring man in this city to allow me to make the following experiment upon him: Three ounces one drachm and a half of blood was taken from his arm, and ſet to cool: he was then put to bed, and a ſweat raiſed upon him by warm whey. After he had continued ſweating profuſely about ſeven hours, he was taken out of bed, the orifice was again opened, and the ſame quantity of blood taken from it as before. When both were perfectly cool, I ſeparated the ſerum of each from its craſſamentum, and having weighed the ſerum of the former, found it exactly one ounce, three drachms, and that of the other one ounce, three drachms, fifteen grains. Theſe proportions of the ſerum to the craſſamentum are, I imagine, leſs than what will generally be found in blood; the reaſon of which, I ſuppoſe, is, becauſe the perſon was accuſtomed to very hard labour; and it is an eſtabliſhed fact, that the blood of labouring people is always *cæt. par.* denſer than that of thoſe perſons who are accuſtomed to indolence and eaſe. What induced me to make this experiment was, becauſe ſeveral authors have imagined that profuſe ſweating drained the blood of its more ſerous parts, and left behind only a viſcid craſſamentum, unfit to enter into, and circulate through, the *vaſa minima*; and therefore have reckoned that it often did harm inſtead of good. But from this experiment, and from a proper examination of the matter, it appears, that there is no real foundation in nature for this opinion; as no ſweat, however brought on, can be long continued, unleſs the ſweating perſon is plentifully ſupplied with ſome diluting liquid; and if we examine the quantity of this liquid made uſe of during the ſweat, we ſhall generally find it very much exceed the quantity which paſſes off that way. It may, indeed, be objected, that a great part of this liquid is diſcharged by urine, and that, during the time of a ſweat, more may paſs off in this way, and through the pores of the ſkin, than is taken into the ſtomach; to determine which, I made the following experiment.

EXPERIMENT VII.

I took two pair of blankets, which weighed exactly ſixteen pounds five drachms, one pair of which I put below, and the other above me on the bed; then having thrown off my ſhirt, and laid me down, I had by my bed-ſide five pounds of warm new whey, of which I took a draught from time to time, and finiſhed the whole of it in about half an hour; and was by that means in a profuſe ſweat. In a little while after I went to ſleep, and did not awake till next morning, when I found the ſweat ſtill upon me, and the blankets very wet. I then aroſe, weighed the two pair of blankets again, and found that they now weighed ſeventeen pounds, eight ounces, and ſix drachms, which was one pound, eight ounces, and a drachm more than they had done before; conſequently they had imbibed that quantity of ſweat. When this was done, I made urine, which I had retained ever ſince I began to drink the whey: this weighed exactly twenty ounces and half a drachm, which, added to the quantity of ſweat imbibed by the blankets, make exactly two pounds, twelve ounces, one drachm and a half; that is, two pounds, four ounces, one drachm and a half leſs than the quantity of whey I had drunk.

Though this method of experimenting is by no means ſo accurate and concluſive as the ſtatical balance of *Sanctorius*; and though it will not ſhew exactly the quantity of ſweat that paſſes through the pores in a given time, when a given quantity of liquor is drank; it, in my opinion, evinces, that the quantity of liquor taken into the ſtomach during a ſweat may always be, and moſt commonly is, much larger than the whole of what goes off by the ſkin and kidnies, taken together. It therefore ſeems plain, that the blood is in no danger of being preternaturally thickened by ſweating, provided that the patient takes a ſufficient quantity of any fluid, to ſupply the place of that which is evacuated.

I took notice above, that the blood of athletic labouring people is generally denſer, and has more craſſamentum than that of the indolent and delicate. As one of the moſt evident cauſes which we can aſſign for this difference, is the vaſt quantities which they almoſt daily ſweat, it would be natural to conclude, that one of the conſtant effects of ſweating ſhould be to render the blood thicker. To clear up this ſeeming difficulty, let it be conſidered, that the method whereby a ſweat is raiſed upon a laborious perſon, and upon a patient in bed, is very different; for in the former it ariſes purely in conſequence of the increaſed muſcular motion, and is always frequently repeated, and often long continued, without any thing to ſupply the place of what is evacuated, which muſt therefore conſiſt of the ſerous part of the blood; and the quantity of theſe ſerous parts, thus daily drained off, exceeding the daily quantity of ingeſta taken to ſupply them, the blood muſt of conſequence be always in a denſe thick ſtate; whereas, in the latter, it ariſes purely by the aſſiſtance of dilution and a requiſite heat, when every muſcle in the body is in a ſtate of reſt and inactivity, and when every particle of the ſweat paſſing through the ſkin is abundantly ſupplied by the liquid commonly preſcribed as the ſudorific. This difference plainly demonſtrates, that though the conſtant ſweating of a labouring man may thicken his blood, yet the method of ſweating a patient in bed cannot have the ſame effect; and this concluſion is ſtrongly corroborated by the ſixth experiment.

Having by the above experiments ſatisfied myſelf concerning the moſt eaſy and expeditious methods of ſweating, and endeavoured to prove that it is an evacuation which is of much more conſequence than has generally been believed; as alſo that it does not thicken the blood, if plentiful dilution is uſed along with it; I next reſolved to try what would be the effect of ſome of the warm methods of ſweating, by volatile-alexipharmac medicines, with very little dilution.

EXPERIMENT VIII.

I prepared three of the following boluſes: ℞. *Pul. Serp. Virgin.* ℈i. *Sal. Volat. Corn. Cerv.* gr. vi. *Syr. Zinzib.* q. s. *ut. f. Bol.* The firſt of theſe I took immediately after I went to bed at night, and at the ſame time applied the thermometer to my ſtomach. In twenty minutes after, I took another, and, after the ſame ſpace, the third; ſo that the whole were taken in forty minutes. From the beginning of this experiment, I had loaded myſelf with a large quantity of bed-cloaths. I felt little effect from the firſt bolus. Some time after I had taken the ſecond, I began to grow pretty warm, and had a conſiderable degree of thirſt; and, not long after I had taken the third, this heat and thirſt became almoſt intolerable. On examining the thermometer, I found the mercury, however, was only riſen to 108, which is two degrees below the heat of a fever; and my pulſe only beat 84 times in a minute. When two hours from the taking the firſt bolus had elapſed, I found the mercury (which I ſtill kept at my ſtomach) had riſen to 112, and my pulſe to about 91. My ſkin was now become exceſſively parched, dry, and hot, and felt hard to the touch; and my thirſt was increaſed ſo much, that I had no longer patience to bear it. I had by my bed-ſide two pounds of tepid water-gruel in a tea-pot, of which I took a pretty large draught, and laid myſelf down again, expecting a ſweat would ſoon appear: but I was diſappointed; for, after I had waited half an hour, I was ſtill as hot and reſtleſs as before. I then took another draught of the gruel, and waited ſome time after, hoping a ſweat would appear, though it did not. The mercury was now riſen to 113 degrees, and my pulſe to about 97. I now took the laſt draught of my two pounds of gruel, laid myſelf down again, and in about half an hour after found my ſkin ſofter to the touch, with a ſmall and almoſt imperceptible degree of moiſture upon it. I expected this would increaſe to a ſweat; but finding it did not, my patience was exhauſted, and I called for another bowl of the water-gruel, of which I took ſeveral large draughts; after which the ſweat ſoon came out plentifully, the thirſt and heat diminiſhed apace, and I ſoon went to ſleep. I reſted tolerably well all night, but the next morning had a dry tongue, ſome thirſt, and a little quickneſs in my pulſe, which all went off after I had drunk a great quantity of tea to breakfaſt.

EXPERIMENT IX.

Some evenings after I repeated the ſame experiment, and the effects were ſimilar to thoſe I have already related, except that now the liquids which I drank to bring out the ſweat (which I had not been able to procure by heat alone) found a vent by the kidnies, and ran off ſo plentifully by urine, that all I could drink had no effect as to producing a ſweat. The family being all in bed, I could get nothing warm to apply to my ſkin, in order to relax it; upon which I covered my head under the cloaths, and in a little while my own breath diffuſed a ſort of moiſture all over me, which brought on a ſweat.

The intention of theſe two laſt experiments was to ſee how far mere heat, and medicines reckoned attenuating, would operate in producing ſweat, without the aſſiſtance of diluents; and, from what happened during them, I dare ſay every unprejudiced reader will agree with me, that the intenſe heat which was excited, rather hindered than promoted the cuticular diſcharge: for, during this heat, two pounds of warm water-gruel were inſufficient to procure any ſweat; whereas, in my ordinary health, half that quantity, any night after I am in bed, will procure it very eaſily. But, from a ſubſequent part of the experiment, it will not only appear, that a hot regimen and heating medicines contribute to hinder ſweat, but alſo that they muſt prove highly detrimental, if not uſed with caution and propriety; for if from a ſtate of perfect health they could throw me into a temporary fever; could raiſe the mercury from about the natural degree of the blood's heat, which is 100, to 113, that is, three degrees above the heat of a common fever; could augment the number of the ſtrokes of my pulſe from about 72 to near 100 in a minute; what muſt they do when preſcribed (as I am afraid they ſometimes are) in the height of an inflammatory fever, when the heat is already by much too great, the pulſe too frequent, and the blood rarefied to a very great degree?

Having now finiſhed the few experiments I intended to make on ſudorifics, and which I thought were neceſſary to clear up ſome doubts which I had long entertained concerning both their modus of action and effects; and having made ſome particular reflections as I came along, I ſhall now conclude this Eſſay with ſome more general ones.

In the firſt place, I am perſuaded, that theſe experiments will make the action of diaphoretics appear in a very different light from that in which it has generally been viewed: for every thing that could procure a ſweat has hitherto been conſidered, by pharmaceutical writers, as doing it either by attenuating the more viſcid fluids, and thereby fitting them to paſs off through the cuticular pores, or by ſtrengthening and ſtimulating the ſolids in ſuch a manner, as to enable them to ſqueeze through theſe pores whatever was already fit for expulſion. But cold water, from what has been related above, appears to be in ſome caſes a very powerful ſudorific, though it certainly has no power of attenuating beyond any other thing that is equally fluid. A piece of warm wet flannel, or a warm vapour, will almoſt inſtantly procure a ſweat; at leaſt it will do it much ſooner than we can reaſonably ſuppoſe any thing, of either the one or the other, to have penetrated far enough into the body to have diſſolved the coheſion of any of its viſcid juices; and, from their being able to procure a ſweat in ſo ſhort a time, it ſeems plain, that it may almoſt always be raiſed without any previous attenuation of the humours, or alteration from their natural ſtate, in a tolerably ſound body. It appears alſo, on the other hand, from a variety of facts collected by different medical authors, that all the humours may be ſurpriſingly thinned and diſſolved by medicines or a diſeaſe, without their having any tendency to eſcape through the cuticular pores. In our endeavours, therefore, to inveſtigate the cauſes of ſweat, ſomething more muſt be taken into the account than mere attenuation or expulſive force; and this certainly is relaxation of the fibres of the ſkin, and a conſequent enlargement of the diameters of its pores. How warm wet flannel, or warm vapour, produces this effect, is obvious to every one; but how cold water ſhould often operate in the ſame manner, ſeems hitherto not to have been fully conſidered or explained.

In order to throw ſome light on this matter, let it be conſidered, that cold water has no power of producing any ſweat, unleſs the heat of the perſon who takes it be at that time conſiderably above the degree which is requiſite for raiſing that evacuation. It is a fact well eſtabliſhed, that while the heat remains conſiderably above that degree, no ſweat can be raiſed but with the greateſt difficulty; the moſt ſolid reaſon that can be given for which is, becauſe then the rapidity of the blood's motion is ſo great, that little or almoſt nothing of its more ſerous parts has time to paſs off by the ſmall lateral veſſels. In this caſe a draught of cold water, or any other cooling fluid, as I proved by a former experiment, leſſens the irritability of the heart, the momentum and velocity of the blood, and ſo allows the ſecretion by the lateral veſſels to go on in its uſual manner; and a conſequent puſh to be made againſt the pores of the ſkin, which now eaſily give way, as the ſtricture upon them occaſioned by the too great heat is removed.

There has never, perhaps, been a more pernicious practice introduced into medicine, than that of imagining a great degree of heat neceſſary to ſweating, which, however, may have taken its riſe from obſervation; for we conſtantly ſee people who labour very hard, ſweat in proportion to the increaſe of that labour, and the heat occaſioned by it. Perſons who work at furnaces, in glaſs-houſes, &c. in a very great degree of heat, generally ſweat while at work, and will often continue to do ſo for hours together, in ſuch a manner, that the ſweat ſhall be almoſt continually dropping from their faces. Theſe appearances, and at the ſame time improper reaſoning concerning them, might, I imagine, originally have given birth to the cuſtom of heating a ſick perſon with all the violence of medicine and cloaths, in order to bring him into ſimilar circumſtances, when a ſweat was wanted. Fatal cuſtom! but too long and firmly eſtabliſhed in the minds of men, to be eradicated by any other means than the united efforts of ſolid reaſoning, confirmed by facts and experience.

In order to ſee how far even obſervation may miſlead mankind, let us conſider the action of heat alone, and we ſhall find it but very ill adapted to produce ſweat, eſpecially that which is cauſed by the internal uſe of medicines. Dry external heat is well known to tighten and corrugate the fibres of the ſkin, and the internal uſe of heating medicines increaſes the irritability of the heart, and momentum of the blood; both which means contribute to hinder perſpiration.

When we view attentively the ſweat of very hard labourers, and of thoſe who work at furnaces, &c. and conſider every circumſtance attending it, we ſhall generally find that it is colliquative, and does not conſiſt chiefly of the ſerous parts of the blood, but alſo of the fat melted down, and excreted along with them. This needs no other proof than the very appearance of ſuch people, as they are commonly lean, withered, and quite exhauſted of all moiſture. The practice, therefore, of endeavouring to raiſe a ſweat, or to keep it up when begun, by exceſſive heat of any ſort, is, to uſe the phraſe of Dr. Huxham, melting, and not mending, your patient: and, from all the obſervations I have hitherto been able to make, I have conſtantly found, that one hour of very profuſe ſweating, in a place greatly heated, weakens a perſon much more than twenty-four hours, when there is only a gentle moiſture on the ſkin. Various reaſons may be alledged for this; but one of the moſt obvious ſurely is, that, in very profuſe ſweats, a conſiderable part of the fat is melted down and evacuated.

Repeated obſervations have likewiſe taught me, that gentle ſweatings, long continued, if the patient at the ſame time be properly ſupported, have infinitely the advantage over thoſe that are larger and ſhorter. The former gradually open obſtructions, and deſtroy the coheſion of viſcid juices, without any great expence of ſtrength; whereas the latter hurt the texture of the ſolids ſo much, that they loſe their elaſticity, and become leſs capable of acting upon the fluids, either by propelling them along their proper canals, or diſſolving any viſcidity they may have contracted.

The power that cold water has of proving an excellent ſudorific in ſome circumſtances, and of immediately ſtopping a ſweat in others, ſeems to point out to us the reaſon why even the moſt approved ſweating medicines will not anſwer at all times, nor upon all perſons, though applied with the greateſt care: for if the ſudorific made uſe of is of the heating kind, and the heat of the perſon who takes it, at the ſame time, too great, it muſt undoubtedly fail of ſucceſs. On the other hand, if it is of the cooling kind, and the heat of the perſon who takes it, at the ſame time, too little, it muſt here alſo fail of ſucceſs. If we would therefore always ſucceed in our endeavours to raiſe a ſweat, we ſhould determine, before we attempt it, whether the patient is then above or below the degree of heat which we find generally moſt conducive to that evacuation. If he is above it, we ſhall ſucceed beſt by cooling and diluting; if he is below it, by heating and diluting.

As the degrees of heat neceſſary for ſweating are very different in different perſons, a difficulty in diſcovering what is the degree neceſſary to bring it upon this perſon, and what upon the other, will often ariſe. This I believe is reducible to no general rule; however, from my own trials and obſervations, I have found, that it is commonly 6, 8, or 10 degrees above what is natural to the conſtitution in perfect health. Thus, for inſtance, if my conſtitutional heat in health is 98, or 100, by raiſing it to 106, or 108, and at the ſame time diluting plentifully, I ſhall procure a ſweat; but if I raiſe it much beyond this degree, I ſhall be ſtill the farther from attaining my wiſhes. When by any diſeaſe my heat is raiſed to 104, or 106, in my endeavour to ſweat, I ſhall perhaps be as hot as 112, or 113, before it appears; and beyond this degree I have never known any ſweat ariſe. When by any diſeaſe the heat is as high as 110, or 112 (which is very rare), then all attempts to procure ſweat, by raiſing it ſtill higher, will prove abortive; and the only probability we have of ſucceeding, is by reducing it.

It will ſometimes happen (which ſeems not a little ſtrange), that when you have brought your patient to what you reckon a proper degree of heat for ſweating (for inſtance 106), and have kept him ſo for a conſiderable time, expecting it in vain; if you augment this heat a few degrees farther, and continue it for half an hour or an hour, and then reduce it again near to the degree of 106, at which you expected it before, the ſweat ſhall come out very eaſily. Is this owing to the removing any obſtruction by the increaſed heat, or to ſome other cauſe?

From theſe obſervations I think it is poſſible to deduce a theory of ſweating, which may eſtabliſh that practice upon more certain principles than have hitherto been laid down. Yet, even proceeding upon theſe principles, we ſhall not always have it in our power to procure a ſweat when we deſire it; ſo very difficult it is to fix any certain *data* to direct us concerning the operation of medicines; and if we find it often difficult, upon any of the principles yet eſtabliſhed, to make ſome people ſweat, it is perhaps ſtill more ſo to aſcertain in what caſes it will be attended with advantage or diſadvantage.

The following corollaries, drawn from experiments and obſervation, may perhaps throw ſome light upon this ſubject.

Corollary 1. When the velocity of the blood is too great, and its momentum too little in proportion, ſweating will generally increaſe the velocity, and diminiſh the momentum.

Corollary 2. When the velocity of the blood is too little, and its momentum too great in proportion, ſweating will generally diminiſh the velocity, and increaſe the momentum.

Corollary 3. When the velocity and momentum of the blood are both too great, ſweating will weaken both; but if it is continued long enough to exhauſt the natural ſtrength, it will then again increaſe the velocity, but not the momentum*.

From theſe corollaries we may form a ſort of general plan when ſweating is uſeful, and when not. Laying it down, therefore, as a poſtulatum, that the ſtrength of nature depends more upon the momentum than upon the velocity of the blood, whenever we find a ſweat increaſing its velocity, and diminiſhing its momentum, we are ſure that it is weakening the patient, and therefore muſt endeavour to ſtop it. Again, when we find a ſweat increaſing the momentum, and diminiſhing the velocity, of the blood, we may be ſure that it is then emptying the over-loaded veſſels, or opening ſome obſtructions, and, in one of theſe ways, adding to the natural ſtrength. Farther, when we find a ſweat diminiſhing the velocity and momentum of the blood, when they are both too great, we have reaſon to believe it is then carrying off ſome morbific matter, which was the cauſe of this augmentation; and may therefore go on with the ſweat almoſt as long as we find the momentum and velocity diminiſh in an equal proportion to each other: for we may be aſſured that, while they do this, nature is never weak, as very few, if any, inſtances ever happen, where great weakneſs is not attended with a very quick pulſe.

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* See Experiment III. as alſo *Dr. Home*'s Medical Facts and Experiments, p. 220, Experiment V.

But though theſe obſervations may ſerve as ſo many rules when to continue a ſweat already begun, they afford us but very little light in determining thoſe caſes in which we ought to order it, or to refrain from it. Nor, indeed, is this an eaſy matter; for every practitioner who is a careful obſerver of nature, and who preſcribes with deliberation and judgment, will ſometimes meet with caſes where he thinks he has the greateſt reaſon to expect ſucceſs from a ſweat, and yet it ſhall do harm; and others where he has been very much afraid of it, and doubtful whether he ſhould order it, and yet it has had very happy effects. But doubts and difficulties will always attend the practice of every ſcience, which has not for its baſis ſome fixed and unalterable rules.

There are ſome caſes which abſolutely require ſweating, and never terminate happily without it; ſuch as the hot fit of an intermitting fever, the various diſorders that happen by a ſtoppage of the perſpiration, &c. There are others again which are generally, though not always, the better for it; ſuch as inflammatory fevers, rheumatiſms, dropſies, &c. And there are a third ſort where it certainly does miſchief: theſe are low, nervous, and putrid fevers, hyſteric and hypochondriac diſtempers, and all caſes where there is a great depreſſion of ſpirits, ariſing from weakneſs or depletion, or a fixed melancholy temper of mind.

Upon the whole, if we obſerve diligently, we ſhall find, that the evacuations by bleeding and ſweating are ſo very ſimilar in their effects, that wherever the former is improper, the latter, if not very cautiouſly managed, will be improper alſo. But as bleeding is a quick operation, and we cannot eaſily aſcertain its effects till the operation be over, we may often be deceived by it: whereas ſweating proceeds by ſlow degrees; and if it is like to do miſchief, that miſchief may eaſily be ſtopped, by putting an end to the ſweat before it has gone too far. Every prudent phyſician, therefore, when he has ordered his patient to be ſweated, and is not perfectly clear that it will at leaſt be ſafe, ought to ſit by him, or at leaſt to viſit him very frequently, and endeavour to diſcover, from the alteration it makes in the momentum and velocity of his blood, whether it ſhould be continued; for if he neglects to do ſo, the foundation of nature may be ſapped before he is aware, and the ſtrength ſo much exhauſted, that no effort ſhall ever be able to recover it.

In bleeding very young children with leeches, the ſame thing ſhould be practiſed; for I have ſeen ſome inſtances where the blood loſt in this way, however trifling we may think it, has brought on a ſurpriſing languor and weakneſs.

But though both bleeding and ſweating occaſion a very great proſtration of ſtrength when they are carried too far, yet that occaſioned by the latter is often greater or leſs, according to the attending circumſtances. Thus if a patient be well ſupported by ſtrong broths and generous wine, he may ſweat a great while, even profuſely, and yet his ſtrength will not ſuffer very remarkably. If he is only ſupplied with water-gruel, whey, or any other weak diluting liquid, it will ſuffer very much: but if he is not ſupplied with any thing either to eat or drink, and a profuſe ſweat be long continued upon him, he will ſuffer infinitely more than in any of the former; becauſe here not only the finer parts of the blood are drained away, and nothing taken to ſupply their place, but alſo a part of the fat melted down and evacuated along with them. And how much a proper quantity of fat conduces to the ſtrength of animals, every day's experience teaches us; as the ſame horſe which, when fat and plump, is able to carry a great load, when reduced to leanneſs ſhall, perhaps, not be able to bear above one half of it.

I ſhall only obſerve farther, that from Experiments VIII. and IX. it appears, that every endeavour to raiſe, or at leaſt to continue a ſweat for any length of time, by dry ſolid medicines, ſuch as boluſes, powders, &c. ſeems at beſt uſeleſs and unavailing; for in ſeveral attempts I have made upon myſelf and other people in this manner, I could never ſucceed without the help of plentiful dilution: nor do I believe it poſſible to ſucceed without it, unleſs the patient be expoſed to a degree of heat ſtrong enough to melt down ſome of his fat. I have alſo made ſeveral attempts to diſcover whether plentiful dilution, joined with dry medicines called ſudorific, had any advantage over plentiful dilution uſed alone: but the reſult of theſe attempts has not yet fully enabled me to determine this matter.

To what has been already ſaid, I ſhall only add three caſes, which ſerve to prove the corollaries which I have drawn from the above-mentioned experiments.

CASE I.

October 9th, 1765. A gentleman of a thin habit of body, aged about twenty, complained of coſtiveneſs, very ſevere gripes, an obtuſe pain in the back part of his head, attended with great dullneſs and dejection of ſpirits. His pulſe beat 87 times in a minute, and was very weak and compreſſible. He was then ordered a vomit, which operated very well.—10th. Symptoms much the ſame as yeſterday, the coſtiveneſs ſtill remaining, for which he had a clyſter in the evening; but as this did not operate, a lenient purging ptiſan was given at night.—11th. The ptiſan had operated very ſeverely, and continued to do ſo. In the evening he took another vomit; pulſe 100.—12th. The purging ſtill continued—tongue brown and dry—ſkin hot and rough—head very confuſed—pulſe 104. An aſtringent mixture, with confect. japon. was ordered.—13th. In the afternoon purging ſtopped, the other ſymptoms as before. A camphorated julep was ordered, and a caſtor bolus in the evening.—14th. Purging returned, pulſe 109—white decoction ordered for his common drink.—15th. Tongue dry and blackiſh—teeth very foul—ſkin exceeding hot and rough. A mixture with tart. emet. was given, which vomited him a little.—16th. Pulſe 110: the white decoction was continued, and in the evening a bliſter applied to the neck.—17th. Pulſe 123—very weak—purging quite gone. He ſlept pretty much, and began to be inſenſible.—18th. A bliſter was applied to each ancle, and the ſolution with the emetic tartar repeated, but without any viſible effect. Pulſe now 129.—19th. In the morning he became exceeding weak—pulſe 136. A muſk julep and claret were ordered to be given frequently. At twelve o'clock, a blanket wrung out of warm water was ordered to be wrapped round his body. Soon after this he fell into a ſweat; his pulſe became more frequent; and at one o'clock it was ſo quick, that it was impoſſible to count it. He died about half an hour after two.

CASE II.

A middle-aged man, of a ſtrong conſtitution, was attacked with a violent fit of ſhivering, pain in the ſmall of his back, loins, and head, with a full, ſtrong, ſlow pulſe, which beat only about 52 in a minute. He was ordered to be blooded; but it being late at night, he would not conſent to it till next morning, leſt his arm ſhould bleed in the night. He was then ordered ſome warm wine whey, of which he drank very liberally till a ſweat began to appear; when his pulſe had riſen to 84, but was much more ſoft and compreſſible than before. After it had continued about an hour, the pulſe was reduced to 75, which he ſaid was nearly what it uſed to beat when he was in perfect health. From this I was induced to think, that the ſweat would be ſufficient to cure him without bleeding, and therefore left him with directions, that a gentle moiſture only ſhould be kept on his ſkin till I ſaw him again. I then went home, and returned the next day about eleven o'clock: his pulſe was then at 70, and all his complaints much abated. He now wanted to be blooded; but upon giving him the reaſons why I thought it unneceſſary, he agreed to them, kept the houſe all that day, and the following was perfectly well, and went out as uſual.

CASE III.

A lady of a very delicate conſtitution had been confined to her room ſeveral days. She complained of great thirſt, pain in her back, loins, and head: her pulſe was full, hard, and frequent, beating about 97 times in a minute. Bleeding was propoſed; but ſhe had ſo great an antipathy to it, that ſhe declared ſhe would rather riſque her life than ſubmit to it. Sweating by warm liquids was not judged ſafe, as it would have greatly augmented both the velocity and momentum of her blood, which were already too high, before it could have appeared. A blanket wrung out of warm water was therefore wrapped round her legs and thighs, and ſome tepid milk and water given her to drink; after which ſhe fell into a ſweat, her pulſe then beating 104, though it ſoon began to decreaſe. She was ordered to be kept in a gentle ſweat all night, and the next morning her pulſe was fallen to 79 or 80. The ſweat was urged a little ſtronger in the forenoon, and at one o'clock her pulſe was at 70. It was then judged proper to deſiſt from puſhing the evacuation any farther, leſt it ſhould reduce her too much. She recovered but very ſlowly, as is often the caſe with people who are exceeding delicate.

FINIS.

ERRATA.

P. 3, l. 19, for *immerſing*, read *immerging*. P. 18, l. 4, for *remedies*, read *treatment*. P. 72, l. 4, for *evacuatory*, read *evacuating*. P. 163, l. 12, after *heat*, add *came*.